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PREFACE.

In the following pages I have endeavoured to *sketch* the principles and application of what I call "Organic Surgery" to the Treatment of Tumours.

The plan pursued is very simple. I have endeavoured to give,

1st. A general idea of the Nature of Tumours.

2ndly. To impress that they must be the result of the actions of one or more of the various organs, on the materials subjected to them.

3rdly. That if the result be unhealthy, the action in a practical sense must be unhealthy also.

4thly. That, in discovering the organ or organs at fault, we must be careful to distinguish health from disease.

5thly. That, to ascertain the causes of the latter, we must enlarge our investigation.

6thly. I have suggested the manner; adding "hints" on the treatment of different organs.

Lastly. I have offered, as illustrations, a *few* of the results I have already obtained.

It is the object of "Organic Surgery" to extend to all the organs of the body just that kind of investigation which, in a recent wound, we apply to the part. Formerly, in such a case, it was thought necessary to use "vulnerary balsams," "detergents," &c. &c. *Now*, we remove all "interfering influences," as glass or other foreign body, gently approximate the divided surfaces, and leave

Nature to perform her own work; in other words, "allow" the wound to heal. In organic surgery, we examine all the *influences* to which the various organs, whether brain or others, have *been at any time subjected, as well* as those at present in operation, and endeavour to adjust the external relations of every organ to its *absolute* power, as well as its *relative* condition to all others. The allegation is, that diseases are the result of organic disturbance*; and, whether they are represented by mechanical or chemical injuries,—the slower but not less certain operations of improper diet, habits, modes of life, particular employments, or moral disturbance,—that the repair, resistance, cure, or by whatever term you phrase the restorative processes, depends on the removal of disturbing influences, which is regarded as the only method of certainly re-establishing the healthy condition of the respective parts or organs. Lastly: That the investigation required is nothing more than common sense suggests, and daily experience proves not only to be *necessary* in other sciences, but productive of *continued* improvement.

I would guard the reader, however, from expecting that I have, in this little book, *exemplified* all the requisitions: people will not read thick octavos; and I can hardly think an apology necessary for not having accomplished that which, in a small volume, appears to me to be impossible. For the *sketch*, and for that only, I would be held responsible; and here I am ready to admit any imperfections. I have felt the greatest difficulty in knowing what to select from bulky manuscripts, at

* That is, in any organized part.

a time when I have been subjected to unusual and unexpected interruptions. The very abundance of materials in my possession has been not a little embarrassing. I may have neither chosen the plan which is best calculated to convey my views, nor the facts which most powerfully support them. I trust, however, I have made myself intelligible ; and I care not how slowly the reader may adopt my impressions, so long as I induce him fairly to try that kind of investigation whence they have arisen. Hasty belief in human affairs is often mischievous ; a reasonable distrust, never.

One branch of enquiry will long be very difficult. I mean that extension of chemical analysis at which I have hinted ; but I have reason to believe that Mr. Maugham, who is certainly eminently qualified for the task, is willing to devote himself to chemical analysis, on such terms as will greatly lessen, if not remove, the present obstacles, which I have mentioned, as connected with this subject.

As I have written entirely from my own practice, I am not aware what assistance we are likely to derive from microscopic enquiries ; but no source should be neglected, till we have *proved it* to be unfruitful.

The events passing around us impressively teach that, whatever rank we are to hold, we must continue to base it on that best of all possible foundations—"public utility." Let us, then, not depend on extraneous help ;—let us not expect *too much* from legislation.

So far as checking the corruption of corporate or collegiate bodies,—opening our public institutions, so that the appointments be not in the pa-

tronage of a few families,—and making the "detur digniori" *really* the only portal through which they are to be obtained,—no doubt Government could render the Public good service, and accelerate the progress of science.

Still we must do *our* parts; and, regardless of authority or conventionalism, reject all erroneous or defective modes of enquiry.

If we reason on a portion of the facts only, our conclusions must necessarily be false or conjectural; but if we collect the *whole* of the facts before we begin to reason, or at all events omit none that are within our power; and if, moreover, we carefully observe the rules laid down by Lord Bacon, the excellence of which has already been verified in every other branch of science; we shall soon obtain a position which no legislature can give, and of which no corruption can deprive us. Our public usefulness will be alone the measure of our rank and value; and these will be in proportion to our success in rightly interpreting the laws of the animal œconomy.

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ON TUMOURS.

CAP. I.

NOMENCLATURE, CLASSIFICATION, ETC.

The word Tumour, in Surgery, is usually employed to designate various substances or growths, which are so placed as to alter the outline or increase the dimensions of the part in which they are seated.

We seldom use the word Tumour in its strictly etymological sense; for, when we regard a part as swollen, we usually add other appellations to express our idea of the nature of the malady, as in various kinds of inflammation, some kinds of dropsy, &c.

The term Tumour is, therefore, objectionable, as really describing no one character of the diseases to which it is commonly applied, since they are not swellings, but consist of newly deposited matter (in or on a part), which is in some sense or other unnatural.

Unnatural deposition, inasmuch as it is an invariable attribute, is therefore the best *general* definition which our present knowledge places at our disposal. Even this definition will include many diseases not *usually* regarded as tumours;

as boil, carbuncle, abscesses, &c. This, however, is unimportant, as the development of the causes of the last-named diseases requires the same extent of investigation, and their treatment the application of precisely similar principles, since they occur under the same law of œconomy, and which I have called the "law of inflammation."

All unnatural depositions result from the disturbance of the function of some organ or organs of the animal œconomy, and are to be treated by the correction of that function. In fact, it is chiefly by the clue afforded by the phenomena of the more common varieties, such as boil, carbuncle, &c. not usually comprehended under the term tumour, that we are enabled to educe the law to which I have just alluded; and which, in its application to tumours, not only exposes their relation to the animal œconomy, but, aided by the mode of investigation about to be proposed, also discloses a more successful treatment of them.

In adopting the term "unnatural deposition," it is necessary to explain the different senses in which that phrase is here employed.

E. G. 1st. Bone may be deposited in or on a bone, or in a part of an entirely different structure. In the former case, it is unnatural in quantity; in the latter, as to its situation. Secondly, depositions are unnatural in the sense of quantity, by which the ordinary dimensions of organs are so exceeded as to constitute what we call hypertrophy or over-nourishment, as in thickening of the muscular structure of the heart or bladder. Lastly, all depositions that present obvious differences from any natural structure, or which merely *simulate*

natural structures in unnatural sites, whether these simply occupy the place of glands or other parts, or whether they occur as absolute additions to the body. It will be to the latter classes that I shall *chiefly* refer, in the following observations—tumours, in fact, in the ordinary acceptation of that term.

The number and variety of unnatural depositions would render a classification of them very desirable; but our present knowledge does not enable us to form one philosophically acceptable. Mr. Abernethy suggested a classification of tumours; and, if the object were simply to point out what tumours were connected with, caused, or accompanied by, seriously deranged states of constitution, and characterized by a fatal tendency; and what tumours were comparatively innoxious, Mr. Abernethy's classification would be nearly all we could desire.

Surgeons,* at the period Mr. Abernethy wrote (and I really cannot perceive that at present they do much otherwise), when a tumour was presented to them, seemed chiefly occupied with the single question, whether it should be removed by operation or not; but it must be obvious to the meanest capacity, that the mere circumstance of whether a tumour was, or was not, usually followed by fatal consequences, could not, *per se*, throw any light on the *cause* of its production; any more than cutting it out could; and for the same reason, that both alike dealt with effects, and effects only.

I would not be understood as undervaluing the

* Himself excepted.

connection, thus proposed by my revered preceptor, between the physical characters of a tumour and its tendency, as dangerous or otherwise to the animal œconomy; no *two* circumstances could be practically more interesting, or theoretically less objectionable. It must always be of the first importance to know the tendency of a disease as malignant or otherwise; and whilst the elements of a scientific nomenclature are wanting, we can hardly base one less objectionably, than on physical appearances; because, in conveying no assumption, it leaves us nothing to unlearn.

Mr. Abernethy's names were for the most part founded on the following circumstances. 1st. Resemblance to natural structure. Thus, tumours resembling the gland of the breast or the pancreas he called respectively Mammary and Pancreatic Sarcoma. 2nd. Physical appearance of the part, as in his Cystic Sarcoma; or its texture, as in Medullary Sarcoma; retaining the old term Carcinoma for ordinary Cancer: but no nomenclature so formed could be made to comprise the multiform structures presented by tumours. In collecting the facts presented in the history of tumours, with a view to elucidate the causes and nature of those depositions, it is the very soul of such an enquiry to render the induction as comprehensive as possible; and, therefore, we should gratefully *include* those characters on which Mr. Abernethy founded his classification as *elements* in such enquiry; but, still, as *elements* only; and perhaps I may have an opportunity of shewing, in the present volume, that they are by no means the most important.

In fact, if we wish to advance our science beyond

a sort of respectable farriery, and really to investigate the causes of tumours, we must enquire much more searchingly into the states of body, whether general or particular, on which they depend; and test our conclusions by the direct experiment of ascertaining whether, on the correction of such conditions, *we can remove the tumours alleged to have been produced by them.*

The enquiry here suggested is not without its difficulties; but they by no means belong intrinsically to the subject. The majority of them are, strictly speaking, extraneous. Much might be said of the negligence of strictly scientific investigation, observable on the part of those who have all appliances and means to boot at their command, and the hardship under which men labour who have little but their enthusiasm to cheer them on through the, alas! too lonely path of inductive enquiry; but it is more to the purpose to regard our respective missions, and for each to endeavour to make himself as useful as he may to his contemporaries, or those who may succeed him.

In the following observations I shall be obliged to be more elementary, perhaps, than may be acceptable to many of my readers; but in all sciences the elements are the things about which we should be clear; besides, I am desirous that every thing should be intelligible to any person of ordinary capacity or education; and, lastly, I am most interested in regard to the younger of my brethren, whether practitioners or students; as those who are most likely to have time to study and *test* the views offered to them. To dictate to my seniors, is no

part of my ambition; and, perhaps, there is some truth in the νεκρον ιατρευειν, καὶ γεροντα νουθετειν, &c.

The human body is made up of certain materials, with the general nature of which we are acquainted; and it depends for its maintenance on the action of certain powers (which we are accustomed to include in the term life) on materials supplied from without. These are, food (whether solid or fluid) and air.

No atom of our bodies can be built up but by a series of processes, which may be expressed in one word, assimilation. The stomach and other portions of the alimentary canal, the organs more immediately subservient to its functions, as the teeth, salivary glands, liver, pancreas and spleen, &c. must all perform their respective duties; the absorbent vessels, and glands, the respiratory and circulating organs, theirs; and, lastly, when the fluidified material (blood) is distributed to the various parts, the powers of the part must convert such material into the characteristic structure, whatever that may be; then again the useless parts of the food, and the old materials of the body, must be removed; thus the absorbents and veins must *return* the blood or other fluid charged with useless matter; and this must, as well as the feculent matter of the food, be conducted to the various outlets of the body, represented chiefly by the lungs, skin, kidney, and bowels. Lastly, the nervous system, whence these various organs derive their power to do *any thing*, must be in a certain state of tranquillity. Now, when we consider that all this is necessary to the deposition of a single atom of healthy structure,

and connect with the view it unfolds the so-called investigations, which are instituted in cases of unnatural deposition; so far from being surprised at the present state of the Surgery of tumours, we shall feel disposed to marvel that *any* tumours have been dispersed, and almost be inclined to adopt the profound remark that I heard a man make, who calls himself a teacher of surgery, when he gravely said, "Gentlemen, the patient in —— ward, from whom we proposed to remove a large tumour, is discharged; the tumour having disappeared *spontaneously!!*" I could present the reader with pages of whimsical specimens of medical philosophy, if "*the mind*" of our profession were represented by persons who constitute themselves teachers under a system, which, though it does not *necessarily* exclude talent, renders it notoriously by no means necessary to the obtaining a tenure of their positions. I have given one; take two more. "These tumours, Gentlemen, we have no power of controlling, and therefore"—What, reader? we endeavour to inculcate more enlarged modes of enquiry into their nature? No—but "*therefore* we remove them!!" Another says, "You have heard, Gentlemen, of cancers being cured—but the fact is, the gentlemen have mistaken the disease."—In other words, cancer is incurable—for, our present science represents the ultima thule at which we can arrive on this subject!!! I make room for one more; because the error is well packed, there being an unusual quantity in a small space. "You may remove enlargements of original tissue by deobstruents!!! and attention to the general health.

But, new formations, morbid growths, cannot be made to *disappear!* After a tumour, properly so called, has been formed, it will go on in spite of all the *applications* you can make; and even harm is done by trying to disperse tumours of this kind," &c. I should think so, if we are to trust to *applications.* I think, too, amongst enlargements of original tissue, that enlarged prostate, many cases of goitre, and hypertrophy of the heart, will be found quite as puzzling as many tumours, "properly so called." I forbear to quote the journals whence these have been extracted.

I have said that the elements of the body are derived from food and air; and, so far as the *material* is concerned, no physiologist thinks of looking to any other sources for the maintenance of healthy, or the correction of diseased, deposition.

But it is very necessary to look further, in regard to the power by which these elements are marshalled, so as to constitute the various structures—I mean the nervous system; and that, too, not only as involving a refined chemistry, not only in regard to the laws, or habitudes, which it exhibits in its government of the bodily functions, but in the relations which it establishes between these and those of the mind also.

With regard to the chemical *habitudes*, we should not identify these with the *powers* of the nervous system. I can conceive no reason for supposing that the chemical *laws* in operation in the body, are in any respect different from those in play out of the body; but it would be extremely unwise to assume, that the chemist's *present know-*

ledge of the latter, represented all those applications of them of which *vital* chemistry is capable. The importance of chemistry in the investigation of the causes of health and disease is sufficiently obvious; yet, chemical, like other modes of enquiry, in order to be useful, exacts great discretion in its application. When it is employed in the analysis of food or air, or of the various substances, solid or fluid, into which the former is converted (whether healthy or diseased) by the assimilating organs, and in disclosing to us the relations of these matters to such as are ejected from the body in health and disease, it is difficult to imagine any source of information more abundant; but when, admitting that oxygen, carbon, hydrogen, and nitrogen may be *usually* dealt with in the body as ultimate principles—I mean that *ordinarily* vital chemistry makes no further analysis of these bodies.—*The chemist* proceeds to regard them as really elements to a vital chemistry, as they all at present are to his powers of analysis: it appears to me to be not only a very unnecessary and improbable, but, in a philosophical point of view, a mischievous assumption. Surely I need only refer to the discoveries of Sir Humphry Davy, in fact to any single page in the history of chemistry, in support of the caution here implied. I venture to say thus much in favour of that view of chemistry which an inductive physiology would alone sanction (I mean that it should not be too exclusively relied on), from a fear that physiology may have received injury from one who intends certainly to be as zealous as he is a distinguished supporter of it—I mean Professor Liebig; for although not insensible, I trust, to the value of his

labours, I cannot but think that, in his application of chemistry to physiology, he has on many occasions neglected those cautions which the ordinary requisitions of philosophy render essential, if we are to be listened to with respect and attention.

He appears to me to have ventured on assumptions not only often unnecessary, but in some instances remarkably unsupported. If I understand him correctly, he regards nitrogen as nearly or quite useless to the body; and really for no better reason, that I can see, than that it *might be* so consistently with *his* views of the uses of oxygen. This almost reminds us of the old experimenters on the spleen; some of whom, it is said, not being able to discover its use, and finding that it could be removed without causing death, gravely determined that it was of no use at all; a decision which they might have arrived at, in regard to the testicle, and ovary, on exactly similar grounds. Some of Professor Liebig's remarks on oxygen, its mode of distribution to the body, &c. appear to me in the light of *very* pure assumptions.

The mode, too, in which Liebig speaks of the development of caloric from the chemical association of carbon and oxygen, would seem not only simply to disregard the multiform processes going on in the body, but would almost lead one to imagine that the development of caloric was something *peculiar* to carbon and oxygen, instead of a phenomenon evinced in every similar change of state in matter, whether this be the act of combination of one body with another, or a change of state after combination;—or a change of state in bodies which to us are *yet* elementary.

That the thousands of daily exemplifications of the relations of carbon to oxygen and caloric should strike the public or amateur, is natural; but that they should be so exclusively regarded as the source of animal heat by Liebig, as they *appear* to be, is to me very strange; first, on account of the caloriferous* phenomena, if I may so speak, developed by every change of matter, and especially as we should expect it to be marked more by the development of sensible heat in bodies which unite, as do carbon and oxygen, without change of volume. I willingly admit "Ne sutor ultra crepidam," and have only ventured so far, from a desire to do what little I can in aid of rendering physiology and pathology a really inductive science.

I honour Liebig; and there is nothing that I desire more than the extended application of chemistry to physiological enquiry. Assumptions from one who has so much of the public ear are very dangerous, and, in promising more than they can perform, tend to make the disappointed blame the inaptitude of the science, rather than the error in its application. In his admirable book on Agriculture, Liebig gives, I think, an illustration of the mode in which chemistry can be applied to physiology with great advantage; for, when he teaches us not to expect from the earth any matters which are not naturally or artificially placed there, or found in the atmosphere, he not only enunciates a neglected truth of great importance, but one which is equally applicable to animal bodies; and which, I

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* I venture to use this word, because all that is true *generally* is, that caloric is in some way affected; that is, appears more or less.

believe, will lead to the true surgery of tumours. Should this prove to be the case, justice requires the admission—that whether we obtain it through the relations of which chemistry may inform us; or through that more refined examination of the various functions, and the injurious influences to which they have at any time been subjected; or, as I would recommend, both in conjunction,—still the one will be a chemical demonstration; the other a more refined and successful application of a *principle* first enunciated by Mr. Abernethy: that the cure of local diseases depended on the tranquillizing the nervous system, and correcting the functions of the digestive organs.

In endeavouring to convey to the reader a clear perception of that which I desire to submit to him, as, in my own view, the true nature of tumours; there are various modes of proceeding; the following appears as simple as any.

If a man weigh, we will say, 150 pounds, and takes four pounds weight of food or drink in the twenty-four hours, why then, if he is to weigh the same, he must get rid somehow or other of four pounds. This is a truism, elementary even to puerility; so much the better. This is a common condition, and the weight and symmetry of the body is preserved.

We know, however, that the weight of men varies very considerably at different times. In youth, the quantity taken in exceeds that ejected, and the body grows. In adult life, also, changes of condition are daily observable, and with some very instructive differences. Let us select two examples for illustration.

Sometimes men become more muscular, sometimes more fat, and sometimes muscular without being fat, and vice versa. Now, when muscle or flesh merely is increased, we usually find the health and general vigour increased also; but, when there is an increase of fat, we, on the contrary, observe the general vigour to be diminished; or, if we do not observe it, we may always render it apparent by any real test of power. We find that fat is composed very largely indeed of charcoal, perhaps not less than 80 per cent.; and we observe also a very constant relation between the quantity of carbon taken into, or, what is the same thing, *retained* in the system, and the tendency to the deposition of fatty matter. In other words, the taking of carbon in excess into the system, or the inactivity of any process attended with its expulsion, is invariably attended by a disposition to fatness. I have no opportunity to go into details; but the fact is every where demonstrable. Fat is very light, and generally deposited first where it is least inconvenient and most portable. It has been thought, from its more constant presence in certain situations, as on the omentum and kidney, that it there serves some *special* function; but this seems very doubtful. Both are very convenient sites for carriage in man and quadrupeds; and, as we shall by and by find, there is little or no fat in either situation in a really healthy animal.

In some animals, fat has very important functions of a healthy kind; but, before we allude to them, let me refer to one other circumstance only:—that sometimes fat is deposited in a distinct site, with a sort of irregular cyst thrown around it, constituting

the fatty tumour of surgeons; this is often removed by operation; which removal, I need scarcely observe, has the same relation to the cause of its deposition, as cutting off the fat from the abdomen of a corpulent man would have to that of obesity. So much then for a common illustration of the relation which exists between the matter taken into the body and *some* unusual depositions.

Hitherto we have spoken of fat as fulfilling a function truly, but one rather constituting disease, or a link in approximation to it; as storing up carbon in the form of animal matter, which had not been separated by the decarbonizing organs, and in a manner least inconvenient to the animal œconomy. That we have so far considered the matter correctly, appears probable, from the fact that fat is never deposited as a really natural habit without a special and intelligible function; as in the provision it forms for hybernating animals. These have the general character of being differently supplied with food at different seasons; at one period there being sufficient and to spare; at others, food being scarce or altogether wanting. Now, in the deficient season, certain animals become torpid; and as this is most common in the winter, though by no means peculiar thereto, they are said to hybernate. Bears, hedgehogs, snails, some birds, insects, reptiles, &c. exemplify this condition. Now all the warm-blooded animals who become torpid in winter, go into hybernation fat, and come out lean; the fat is slowly absorbed and converted to the preservation of the animal: for which its nutritious composition, and probably bad conducting power of heat, alike qualify it.

But these very circumstances shew that such provision is not necessary to animals otherwise situated; and if we did not know the fact from the thousand notorious ill consequences of hypernutrition, we might safely infer mischief; for it is axiomatic, that whatever is unnecessary to us has a tendency to be injurious.

Now, what I wish to urge, is, that enquiry into the nature of a tumour should always recognize the possibility of a relation existing between the chemical elements of the tumour and an imperfect performance of some function dealing with such elements. It will be seen that I have not yet arrived at the ascertainment of such relation in the chemical sense; but I believe that I am approaching that point; and, from the results I have experienced in examining more generally the relation of function with the occurrence of tumour, I have the highest hopes of still better things from extended investigation, if I can only get help to enable *me*, or can induce *others*, to pursue it.

In obtaining comprehensive views of the nature of tumours, there are other circumstances to be considered; and a very important class is represented by those cases in which the increased deposition consists of enlargement of the natural structure of the part. In some of these cases, we are not left to infer, but we can demonstrate, the nature and presence of a functional necessity. The heart, for example, is employed in pumping the blood to all parts of the body; the urinary bladder in ejecting its respective fluid: if the canals in either case be narrowed or obstructed, the thickening, in either case, of the muscular structure of the organ (in

which the expulsive power resides) is an invariable consequence: now here the remedy would be, of course, the removal of the obstruction; but this, in regard to the heart, we cannot accomplish; but we can, in many cases, in respect to the urinary organs, and we can at least say that every evidence of it disappears; though, where patients get well, we can never, of course, absolutely demonstrate that the bladder had been thickened.

Proceeding in this way, link by link, we arrive at enlargements of glandular structures, as the thyroid gland and prostate. Both these parts are subject to diseased increase of other kinds; but the thyroid gland in the well-known goitre represents merely an enlarged view of the natural structure; and the prostate gland *most frequently*, when enlarged, represents a similar condition; I mean enlargement of its natural structure. In short, we have every reason to believe that the habit to which I am referring prevails through the whole œconomy; and that, within certain limits alike applicable to all vital phenomena, an increase of function is always attended by an increase of structure; and that the investigation of the circumstances, out of which the one has arisen, is the only sure and reasonable way of obviating the necessity for the other. For, whether a muscle enlarge, as in a blacksmith's arm; from training and high condition; or as a diseased heart or bladder; or a gland enlarge, as in goitre or diseased prostate; we shall, if I mistake not, find that it is an attempt to meet some increased function or duty, to which the organ in its normal condition is not adequate. This may certainly be the result either of an increase of the natural duty, or of dimi-

nished power from other injurious influences in the organ; a very important distinction in practice, but still leaving the act of the relation of function and structure unshaken.

The more we try this view, the more I think we shall be disposed to base our enquiry on the allegation implied. In the goitre and enlarged prostate, we will not assert that our ignorance of the functions of these bodies is the *sole* cause of the little power we have over their diseases. Still the two facts are indisputable, and their connection general and important; whilst, in regard to any organs, concerning whose functions we *are* at all conversant (for we know scarcely one *perfectly*), we have very considerable power. When the thyroid gland is enlarged, no man thinks that it is unphilosophical to assume that it executes some purpose; because he does so on precisely the same grounds that he conceives the thyroid, in its *natural* state, to have any function at all. What I mean is, that his ignorance of what the function may be, is never pleaded in opposition to the assertion that it has *some* function, or to any enquiry as to its nature. Surgeons, however, seem hitherto to have made little practical application of this to the investigation of tumours.

There is in fact scarcely any deposition which does not, by some evidence of peculiar relations, suggest that the causes are different in different cases, and thus invite enquiry.

Thus, in the cases just referred to of the thyroid, and prostate, and heart, or in similar conditions of bone, matter seems laid down every where in the

substance at the same time (interstitial growth, as it is called); at other times, it is merely heaped up on the outside, as in those enlargements of bone called exostoses. Those cases in which ligamentous matter is laid down instead of bone, whilst they illustrate the functional relations of unnatural deposition, shew us *how* little interference with the conditions necessary to absolutely *healthy* structure seem sufficient to occasion some departure from it. The cases in which we get ligamentous union after fracture, are all cases in which, from bad surgery, defective apparatus, &c., or intention (as to guard against impaired action), motion is employed before repair is completed; as in fractures of the olecranon and patella; and in the plan pursued by some surgeons, until very recently, in fractured neck of the thigh bone within the capsule of the joint, under the erroneous assumption, so long taught by Sir A. Cooper and opposed by Mr. Abernethy, that bony union was in this accident nearly impossible; nor is it unimportant, in relation to the *function* of deposition, that bone is in these cases replaced by the best substitute (ligament).

The constant relation of unnatural depositions to the state of the œconomy is further impressed on us by the remarkable fact (and which applies to every form which I have examined in this respect, whether boil, carbuncle, or tumour, more commonly so called), viz. that they undergo manifest changes *contemporaneously* with changes induced in the animal œconomy; I mean that a boil-like or carbunculoid inflammation of the most unequivocal character may, in certain cases, be deprived of all its ordinary

characteristics, and be converted into common abscess of a healthy character. I have seen this in repeated instances; and in one case, in the most severe carbuncle I ever witnessed. Every one is familiar with the fact that tumours almost certainly of a structure not ordinarily attended by danger, will sometimes assume, and in certain cases very suddenly too, a truly malignant character; as that of cancer, fungus hæmatodes, &c.

Again—it is certain that malignant tumours do undergo changes, both as to *size* and structure. Had I space, I could mention examples of this also. I perceive, however, I shall find it difficult to observe the limits which I have prescribed to these observations. I must therefore forego the pleasure of multiplying the foregoing remarks, and endeavour to sum up in an aphoristic form such of the characters of unnatural depositions, both general and special, as appear to me in a practical sense most important or most essential to the clear enunciation of the views which I am desirous of submitting to the reader.

General Characteristics of Tumours.

1. All tumours are, either as regards site, material, or quantity, unnatural depositions.

2. They must be deposited either directly or indirectly from the blood. The blood itself derives the *materials* for its composition from the food, partly by direct assimilation, and partly from the secondary conversion of the various tissues, and from exposure to air.

3. The surrounding structures of the part in

which tumours are seated remaining the same, it follows that either more blood is sent to that part, or that less returns; that is, it is minus the material employed in the formation of (and constituting) the tumour.

4. All kinds of tumours in different cases exhibit one or other, or all, of the following changes:

The sensibility is sometimes like that of the surrounding structures; sometimes less; at others, greater: and these changes range from comparative insensibility to sensibility productive of acute suffering. These varieties are also, at different periods, sometimes observable *in the same tumour;* the most painful being at times easy, as Cancer; and the most insensible occasionally evincing exalted sensibility, as Bony Tumours, and several others.

All those depositions usually understood by the term Tumours, when compared with the depositions expressed by the terms Inflammation, Boil, Erysipelas, or even Carbuncle, take place much more slowly. Their increase is, in fact, gradual. They *do not* however, increase equally in equal times: sometimes they are for a considerable period, even for years, stationary; sometimes their growth is gradual and progressive; at others, characterized by extreme activity. Cæteris paribus, the prevailing tendency in tumours is to increase; and this is usually more remarkable as their duration has been prolonged.

All depositions occasionally exhibit changes in their physical character, evincing alteration of structure.

E. G. Scrophulous and other depositions often suppurate, or form pus. Cartilaginous, fleshy, or at

all events softer tumours, have bone deposited in them. Abscesses, which had pus evacuated from them, sometimes become filled by straw-coloured serum, of which I have seen remarkable instances. Encysted tumours occasionally evince unequivocal change in the physical characters of their contents. Tumours which have at one period presented characters of an entirely different kind, do sometimes, and in some cases very suddenly too, exhibit those known as of a malignant character; hitherto the change has been observed as proceeding from the more benign to the more malignant; and although it has been *most remarkable* in tumours of some duration, yet there is no necessary connection between such change and the duration of the unnatural deposition. Regarding these changes as evidence of great consequence, I have here connected them with the *general* properties of tumours; because we are not enabled to connect them in any one case with any one elementary form; to which we can say that this or that change is peculiar.

5. All depositions are organized bodies; or produced from an organized surface; as encysted tumours, depositions *on* bone, in sheaths of tendons, &c.; and whether they be one or the other, they are at all events (so long as they are not exposed, by ulceration or other causes, to the atmosphere*) under the influence of the operations of the animal œconomy.

6. That, to sustain the term unnatural deposition, even in the senses before explained, it is absolutely necessary to admit it to be demonstrative of some condition of the body, or part of the body,

* I have made this reservation; because otherwise the generalization in a few very rare conditions *might* be disputable.

which is unhealthy. In fact, the occurrence of unnatural deposition, and an unnatural state of the body, are convertible forms of expression.

7. Unnatural depositions occur at all periods of life.

8. No physiological reason can be assigned why the same laws which lead to the unnatural deposition might not be converted into the means of its removal; and that, though it may as yet be unsafe to assume that experience has exemplified the fact in regard to every species of tumour, it is equally unsafe to assert the contrary, in an absolute sense, in regard to any tumour whatever.

9. The original *cause* of no tumour can possibly reside in itself; because the cause must necessarily have *preceded* the tumour. That therefore the removal of any tumour by operation (excision) is perfectly compatible with the persistence of the cause whence it may have arisen.

10. That, consistently with the foregoing, we can never predict, in any one case, that the removal of tumours by incision may not be followed by a recurrence of the malady, or its *replacement by some other form of disease*—a position abundantly confirmed by experience.

11. That there is a manifest tendency in the œconomy to place all unnatural depositions near the surface of the body, and in a structure (cellular tissue) where they interfere least with important functions. An incalculable majority being placed on or immediately under the skin; and in this way cutaneous diseases, and those diseases the immediate object of the present enquiry, form a link in a regular series of phenomena in perfect accordance

with, and in fact remarkably exemplifying both the common character of such depositions, and that persistent tendency in disease towards the surface, whence I have elsewhere deduced the "Law of Inflammation*."

12. That, although many cases have occurred of tumours having been removed by the natural powers of the œconomy, yet that the precise conditions or alterations of condition of the œconomy, under which they have been so removed, have not been observed; and that, whether as a consequence of this inattention precluding us from becoming acquainted with the operations of nature, or not, the attempts to excite such processes on the part of the animal œconomy,—in other words, to remove tumours by any other mode than by excision,—has been (notwithstanding some exceptions), on the whole, signally unsuccessful.

13. Notwithstanding the alleged tendency of tumours to the surface of the body, that no situation is exempt from such depositions. That however it is observable that those depositions are most prone to internal situations, which are either most serious when seated elsewhere, or which are most strongly marked by accompanying disturbance of the general œconomy, as cancer, fungus hæmatodes, scrophula, &c.

The foregoing are the principal general characters I wish to impress on the reader; some other points are supplied by the special characters of tumours, to some of the more important of which I will next proceed; I have no opportunity here to enter into the history of the several varieties of tu-

* See "Medicine and Surgery One Inductive Science."

mours, although I may probably do so in a future volume.

Of some of the Varieties observed in Tumours.

Some tumours are hard, as cancer in its early stage; and which we simply call scirrhus; or as bony, cartilaginous, or horny tumours. Others are soft, as medullary sarcoma, fungus hæmatodes, melanosis; or pultaceous, as atheromatous, steatomatous, and other encysted tumours (one variety of which is the common wen). Then there are various tumours of intermediate character, as fatty, fibrous tumours, &c.

Other tumours contain fluid, and this may consist of hydatids; or the fluid may be serous, puriform, or blood-coloured; and this may be the purple hue of venous, or the bright scarlet of arterial blood. Some tumours are pulsatile, and thus simulate a disease of an artery (aneurism); and this pulsation may be an intrinsic character, as in the deep-seated vascular tumour sometimes called "aneurism by anastomosis;" or may merely result from the contiguity of a tumour to an artery. Some are accompanied by alterations in the colour of the skin covering them; and this may be deep or superficial, as in the different kinds of nævi, or mother's marks as they are popularly termed. Tumours are sometimes identical with some known structure, as fat; sometimes they present very striking *resemblances* to natural structures,—a feature that may not have been sufficiently attended to, and which in an advanced state of science may prove a clue to the discovery of the disorder of the œconomy whence they arise.

Some are gelatinous; the tumours to which we give the names of fungus hæmatodes and medullary sarcoma, often contain portions which strikingly resemble the matter of the brain; others present appearances resembling the mammary gland, or the pancreas or sweet-bread, which suggested to Mr. Abernethy the names, already mentioned, of Pancreatic and Mammary Sarcoma. Indeed, the varieties of the material found in tumours and other unnatural depositions are endless. Not only are there found the usual elementary bodies in them, carbon, &c., but they are frequently so associated as to form the proximate principles of the body, as bone, &c.; so that I know of no one material of the body which has not been found in tumours, or which has not been so simulated as strongly to suggest similarity of composition. In addition to those already mentioned, hair, teeth, cholesterine (the principle of bile), are amongst matters formed in tumours. *Ordinarily*, their physical arrangement is strikingly different from any known structure.

Tumours are generally deposited near the surface, as already stated, and in a structure where they least interfere with the functions of the œconomy, namely, the connecting or cellular tissue; *comparatively*, seldom in any other structure. Yet there is a remarkable difference observable in this respect in different varieties. Thus some tumours evince a greater tendency to occupy the sites of other organs,—a remarkable characteristic, for example, in the present recognized forms of scrophula and cancer.

Between these two, there is, however, a notable difference; cancer very frequently attacks, and is

sometimes confined to, organs which either have performed the functions assigned to them, or which appear to have been incapable of doing so; as when it attacks the mamma and uterus in women after the period of child-bearing, or in women who have never borne children*. Scrophula attacks all parts, and very *early* the most important organs of the body, as absorbent glands, lungs, &c.

Although it by no means appears that a tumour is *necessarily* dangerous, which occupies the site of a gland or any other organ, yet there is an observable relation between tumours which evince this tendency and the disturbance of the animal œconomy by which they are accompanied, or of which they are themselves a certain indication. This is very striking, when we compare the histories of scrophula or cancer with those of fatty, cellular, or encysted tumours.

Although no age is exempt from these unnatural depositions in some form or other, yet there is an observable relation between different tumours and the periods of life at which they occur, so as to be greatly characteristic of such tumours respectively. Thus scrophula, nævi, and perhaps most varieties of vascular tumour, are most common to the young; cancerous depositions, in those of advancing life. Whilst, in regard to most other forms of tumour, we can scarcely say more than that they are most frequent in adult and advancing life, say from thirty years and upwards.

On the whole, there appears little doubt but

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* I have seen one case of scirrhus in a young woman, who very quickly had rather a large family.

that females are more subject to those depositions, usually understood in the term tumour, than males; but I am not prepared to say how far this would hold, if cancerous affections were deducted; to which they certainly are more liable.

Nothing appears more certain, than that the state of the body is extremely different in different cases of unnatural deposition; that very often the health appears to be so good, that nothing wrong can be detected by our *ordinary* examination; and that the erring function, whatever it may be, is relieved somehow or other by that represented by the tumour.

But in all cases, the foregoing included, that more analytical and inductive enquiry, which it is the special object of the following pages to recommend, scarcely ever fails to obtain evidence of unhealthy actions; almost always developing the occurrence of habits, former diseases, treatment, or tendencies, demonstrating the past or present operation of injurious influences on the body, of well-ascertained character; and which conduct us with great success to the organ on which they have chiefly and primarily acted. And that although, after the *ordinary* examination to which patients are usually subjected, the viscera have been found diseased after death, without any suspicion of such disease having existed; yet that this almost never happens in cases which have been subjected to that examination and tests which will be shortly laid before the reader.

Although tumours are generally so deposited from the blood as to represent a new arrangement of matter, yet some appear to consist of this fluid but little changed in its properties. Again,

in some vascular tumours, we merely have a congeries of vessels, representing in tubes just what a reservoir would be for the reception of redundant sanguification. I have seen encysted fluids so strikingly resembling blood, both as to the arterial and as to the venous colour, as not to be distinguishable from blood, except by the absence of coagulation. Such tumours have, so far as I have observed, been connected with disordered states of health, either very marked, or easily elicited by enquiry.

A minute examination into the histories and relations of tumours, with unnatural depositions generally, and with each other, strongly suggests that the attempts to except cancer, fungus hæmatodes, and other malignant tumours, from that view which supposes that there is nothing impossible in the removal of every tumour by the powers which deposited it, is a pure assumption, founded on a neglect of that extensive investigation from which we can alone expect to obtain positive knowledge; and that such assumption of incurability, on the part of teachers, instead of encouraging more refined enquiry, is calculated to prevent and discourage the labours of the industriously inclined; and to foster that quackery, of which the alleged incurability of such tumours is the real foundation.

CAP. II.

OF THE CAUSES OF TUMOURS.

In the foregoing propositions in regard to the general or particular characters of tumours, I have been obliged to limit myself to such points as appeared indispensable; but these, in a properly constructed argument, should, of course, have been proved. To many of these, I trust the proof is unnecessary; and, if there be any that appear doubtful, I would respectfully beg my reader to believe that I have not hazarded any proposition which I do not think borne out by the present known facts.

In the course of the work, he may meet with opinions which I hold more lightly, because it is not in my power to collect *all* the facts known in relation to the subject; as the comparative importance of the adjustment of carbon, &c. The real difficulty, in regard both to propositions and opinions, is the bulk, which the enumeration of the whole of the facts whence they are deduced would render necessary. As writing large books is out of the question, an author must, therefore, submit, apparently at least, to rest his positions often on an imperfect induction, or on the knowledge of the reader.

Strictly speaking, we are ignorant of the causes of tumours; at the same time, by a careful observation of the facts by which they are preceded, accompanied, and followed, we can educe very important general propositions. 1st. that they are connected with *certain* conditions of the system; 2nd. that these are conditions of disorder, and that, in many cases, we can arrive at correct general ideas of the *nature* of such disorder; because we find that, in such cases, the removal of such disorder is accompanied by the absorption of the tumour.

If we look around us, either in the body, or out of the body, we shall see that all the changes which matter undergoes, are represented by a result, or tertium quid, consequent on two or more forces acting on each other. Any science, chemistry especially, will illustrate this; but let us take physiology.

Food is put into a living body, and a portion of it is converted into muscle, for example. The chemical affinities which held it together as food, represent one force or power; the digestive and other organs of assimilation, another force; and the muscles formed out of these natural actions, a result or tertium quid.

Supposing, then, that I have thousands of examples, shewing that the proper and ordinary function of the body is to secrete, or build up, parts of bone, muscle, tendon, skin, &c.; and to eject certain other forms of matter; and that I see a man with a huge bump on his face, knowing that it is not a natural appendage to him, and if, on examination, I find that, whether it is like the ordinary structures or not, it is in a situation where it cannot serve the

purposes to which they are applied; then, however ignorant I may be, in the *strict* sense of the term, of the *cause* of that bump, I am at least certain of this, that either the food contains something unusual, or that some of the assimilating organs are acting on it in some unusual manner, or both.

This seems indisputable. It does not indeed unveil to us the causes of tumours; but it immediately points to at least one mode of looking for them. That is to examine the food, the various organs which represent the various stages, which I have included in the word assimilation; and to test the properties of both one and the other by any knowledge we may have of their natural effects, habits, and propensities, respectively. This is just what we should do with a manufacture; if a raw material,—wool, for example,—were put into an elaborate machinery, which carried it through various processes, which were to result in the production of a beautiful cloth; and we were to find that the cloth, instead of exhibiting its usual texture, was disfigured by various irregularities; the manufacturer would at once conclude either that the fault was in the raw material, the machinery to which it had been submitted, or both. But we know that, in examining this, he would not examine *solely* either one or the other, much less would he confine his examination to one point in the machinery; least of all would he seek to correct the error either in the wool or the machinery, by throwing in oil, or dye, or iodine, or any other substance, until he had instituted the examination in question. He would, on the contrary, institute a thorough examination until he had discovered and tested the cause of the defect.

He would never think it incurable until he had examined all points. Even should he seek to improve his bad cloth by some special proceeding, he would not the less continue to seek the cause of it, knowing well enough that heretofore the wool and machinery had produced the cloth as it ought to be. Now, *so far* the analogy of the human body is close and perfect. The food and air are the raw material; the organs under a vital impulse are the machinery; and the result is the structure of our bodies; and what we require is, that our investigation should correspond to the obvious and reasonable requisitions which these elementary truths so plainly establish.

We shall never arrive at the causes of tumours by lotions, or plaisters, or "applications," nor even by throwing into the body mercury or iodine, or other things in countless forms of combination; for even though now and then, in the midst of all this firing at random, the tumour should "disappear," we are just as wise as we were before; for in the very next case, perhaps, we find the whole prove ineffectual: so of burning and cutting them out. It seems absurd to think such modes of proceeding can accomplish our object; and even though it were not, surely a sufficient number of centuries have been spent in this way to justify a little impatience for some more enlarged plan of enquiry. To such an enquiry it is that I wish to invite attention; and my own experience convinces me that even a rude approximation to it may be attended in many cases not only with very gratifying and unexpected results, but lead to others, at no very remote period, which will not only disclose to us the true

relations of tumours, but render them the means of throwing light on many other points of disease.

It must be admitted, however, that all analogies drawn from inanimate bodies, or machinery, fail to illustrate the *whole* of the processes going on in a living body. There is intention in all the processes of nature; and, in regard to tumours, this intention, or function as we call it, for they are convertible forms of expression, leads us often very directly to the proximate cause of the disease. So that the investigation of tumours should be from the food, through the different organs, to the tumour, coupled with any of the points suggested by matters excreted from the body, or by the mechanical or physical structure or relations of the tumour. In this way I may see one point very plainly suggested by one form of deposition, which is very obscurely shadowed forth or scarcely perceptible in another.

Now, that tumours represent some function, is indisputable. Viewing them as only one class of unnatural depositions, let us examine those forms which link them most nearly to natural structures: such as hypertrophied organs; enlarged muscular structure of the heart and bladder; and again in the goitre (enlargement of the thyroid) and prostate. Here we see unnatural deposition truly, but under circumstances that compel us to admit that it serves some function; because, if I analyse the matter, I find myself thus compelled, exactly on the same grounds that I suppose the respective organs to have any function at all. I may not, indeed, know what that function is; that is again another question; but the conviction that it fulfils some definite purpose is important, and is precisely what Lord Bacon tried to

impress with regard to natural phenomena generally. Well, I cannot find out that function; but here I find that there is a great deal of difference in the several cases. I know not, for instance, what the natural uses of the thyroid gland or prostate are; and I am equally in the dark as to the use of the respective enlargements of them: but, in regard to the heart and bladder, I am in a somewhat different position: I know, for example, that their functions are—the one to pump forth the blood, the other to eject the urine; and the very knowledge that it is in the muscle that this power resides, suggests an increase of power as the proximate requisition established in the organ. Reflection readily suggests that this increase in a projecting organ can only result from its having more fluid to project, or obstacles to overcome; and accordingly we find that these severally represent the conditions which give rise to the disease; that is to say, a narrowing of the canals through which they eject their respective fluids, or, as in the heart too often, more fluid is sent to it than there ought to be. Now, the treatment of the disease shews that, in the case of the heart, we can indefinitely postpone fatal consequences; and where we can control the conditions, as in the urinary passages*, we can cure the disease altogether.

These cases also shew that the very diseases were so far conservative, that, had not the changes taken place, neither organ would have overcome the

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* I do not allude to the *ulceration* of the urethra in cases of stricture, &c. This would merely change the form of the proportion into one not pertinent to the matter.

obstacle. Now, as neither the ejection of the urine nor the distribution of the blood can be dispensed with, both conditions requiring this increase of power would have been necessarily fatal: so much for the use of morbid deposition, and for the connexion which exists between the perception of that use and the suggestion of a remedy. Indeed, there is no tumour, concerning which we have the smallest notion, that does not more or less shew the same thing—I mean that the ascertainment of their relation to the œconomy, is the only safe mode of educing the means by which they are to be removed. Let us briefly review the facts presented by an aneurism. And here we shall see how beautifully even the simple coagulation of the blood demonstrates function, and how it led Mr. Hunter to one of the most remarkable improvements in practice. The *internal coat* of an artery gives way, and the blood is consequently impelled against the external and yielding coats. These are gradually extended, as it were, into a sort of sac. The giving way of the other coats is the danger; but as the vessel yields at that point, mark what happens—the blood traversing this sac, out of the current or tide, as it were, is *retarded* by the necessary friction; as a consequence of this, a portion of the blood coagulates, and thus interposes layer after layer of fibrin between the blood and the yielding coat of the diseased vessel, until at length it *impedes* the enlargement of the tumour, filling it more or less with coagulum. Now then a new relation occurs: the very *size* and *weight* of the tumour tend to produce a curative, as the arrangement just referred to did a protective, power. The weight of the tumour thus occupied by coagula

presses on the artery above the point where it had given way, and in many cases causes its obliteration. It was the observation of this process, coupled with the fact that the diseased condition of an artery did not necessarily extend beyond the aneurism, which led Mr. Hunter to the improvement to which I have adverted.

If we now continue our observation, we shall cease to find, perhaps, unnatural depositions represented by simple enlargement of organs; but still we find many, obviously consisting of the same materials as are employed in the construction of the body, as bone, cartilage; *many* indisputably having all the materials of a living part, and therefore affording the same grounds for the inference of a particular function. Some, on the other hand, appear unorganized; but then they are contained in cysts which *are;* and, as this is a plan pursued sometimes by nature in regard to foreign bodies, so that, although retained, they may be said to be exterior to the body, it may *possibly* happen that many of such tumours may execute an excretory office merely; still there are none of any kind which more perfectly (I speak of the common wen) exemplify their definite relations with the body: by their occasional absorption; by alteration of their contents; and, lastly, in the serious disturbance which has occasionally arisen from irritation of them.

It is true that we at length come to many curious depositions, and very unlike any thing in the body; but here we need not rely on the failure in the product only, to convince us that the machinery is out of order: we may fail to elicit the precise relations of such depositions; but we see much

that is instructive. We never fail to find plenty of evidence of impaired organs, plenty of proof that even ordinary food is out of relation to the powers of the œconomy; that the nervous system, which should properly impel the whole, is disturbed; and that the power it confers must be kept from all perceptible disturbance, whilst its particular manifestations of weakness must be traced and tested through every organ, to see where the fault is, and in what it consists; to direct thither our corrective, whether it involve the addition or avoidance of any influence; and, if indeed we can no longer obtain the ordinary functions of health, to "accommodate the agenda" to the power.

I wish I could inspire the reader with my own conviction of how necessary the kind of investigation is to which I here allude, if ever we are to raise our profession to a proper station, either as regard the moral or physical benefits it is capable of conferring on mankind. How superior an advance of knowledge would be to all measures directed merely to the reform of our medical polity! How essential it is, if we are ever to place our pretensions before an uninformed public in a simple, definite, and intelligible form, that we should pursue our inquiries in the same manner, and place our results exactly on the same grounds, that are observed in other sciences. The ignorance of the public is, after all, the corner-stone of medical abuses. How can the public judge of men in any other way than by the positions they occupy, if we neither cultivate our science, nor diffuse its principles, in a manner so as to give them better data to proceed on? Once let medical science be properly cultivated, and the public will

soon distinguish science from conjecture. Our protection from ignorance and quackery need no longer seek to rest itself on legislative restrictions; pretenders would be excluded, for the same reasons that they are from chemistry or any other science.

To return—No such investigation as that above alluded to, so far as I know, is taught or practised; and to expect to discover the causes by any thing short of it, appears as absurd as our resting quietly satisfied with a surgery which scarcely recognizes any other remedy but excision as unjustifiable. I shall presently point out the mode of instituting the inquiry I recommend, so far as I have carried it; and the means by which it might be indefinitely improved.

Another point in connection with the cause of tumours, and one which has also important bearings on practice, is the uniformity with which all parts are removed, so soon as the circumstances requiring them cease to exist.

The removal of dead parts, for example, either by their absorption, or by the more commonly observable plan of cutting off the living parts from around them. In aneurism also, you no sooner tie the artery, than the tumour becomes absorbed; and, considering the nature and contents of the sac, it is a very curious example. There is hardly a single successful piece of surgery which does not exemplify the same principle. How beautiful the cure of those complicated fistulæ connected with the urinary organs, on the simple restoration of the natural canal. The wasting of muscles from inaction, their increase on exercise, is as it were a crucial experiment, shewing the constant relation of structure

to function, or, if you please, the removal of a part when it ceases to execute any purpose.

Simple as this appears, still there are many parts of surgery in which gratuitous and demonstrably unfounded assumption is allowed to direct the treatment, rather than an enquiry into the real causes, or an adoption of the simple suggestions of nature. A vast number of the fatal cases of strangulated hernia appear to me to be so many terrible examples of the triumph of an unreflecting conventionalism over the suggestions of common sense; and I speak of a disease of which I have seen as much as most people.

The aperture through which the bowel has been extruded binds on it like a ligature; common sense would direct that you should liberate the bowel as speedily as possible by relaxing the parts connected with the aperture, or its enlargement by incision. This indeed constitutes part of the treatment, and the whole of the operation. But to this are added, both before and afterwards, purgatives; and often a variety of other measures, many of them calculated to do mischief; and this too, notwithstanding that the extraordinary success of contrary practice has been insisted on and exemplified by men of great experience; as I endeavoured to shew in a paper to the Royal Medico-Chirurgical Society three years ago.

Relation to the Law of Inflammation.

When, however, we arrive at the conviction that tumours have special functions, or some special uses, no doubt the still greater difficulty remains of determining what these uses are: but here again we shall obtain advantage in determining their *general* relations. In deducing what I have called the law of inflammation, I have shewn that inflammation is, in fact, only one mode, amongst many others, to rid the body of noxious influences, or to repair injury; and that, whilst the *most natural means* to accomplish the former consist in increased activity in some natural outlet of the body, in inflammation a special process is set up; not so perfect a relief as by means of secretion or excretion; but still a process having a greatly conservative tendency,—that tendency being to seat itself near the surface of the body; and that, although many phenomena occur which at first sight appear to be exceptions to this law, yet, when viewed in all their relations, they turn out to be remarkable confirmations of it. E. G. The internal viscera are sometimes inflamed, truly; but, for one case in which inflammation attacks the viscera, we have perhaps one thousand attacking the membranes covering them, and so on. But of the tendency to the surface *absolutely*, we have hundreds of exemplifications, as in diseases of the skin. But when we have exhausted these myriads of examples, *more numerous far than all other diseases put together*, and proceed from the surface inwards, we find that we arrive at tumours; which are next n order of frequency of occurrence; and which, ex-

emplifying still the tendency to surface, are, in by far the greater number of instances, placed in that situation : so situated, in fact, as to general site and the particular structure they occupy (cellular tissue), as to interfere least with important functions.

Now the law to which I have alluded, and which deduced the true relations of inflammation, —not from characters, one or other of which may or may not be present, but from one which is invariable, viz. unnatural deposition,—necessarily brought within the same category many things not at all included in the term inflammation, but presenting the common character which I have mentioned. In following out that law*, imperfect examples of it were referred to interferences, collectively designated as "want of power," which want of power was referred to disorder of some portion or of the whole of the animal œconomy ; just as digestion might be said to be the law of the stomach, although there are many interferences with its execution, represented collectively by cases of indigestion.

Now, to bring tumours under that law, we have to enquire whether their tendencies as to site are to the surface, whether this tendency in general is salutary, both of which are obvious ; and whether we can obtain a practical evidence that they are consequent on, or coincident with, disturbed function.

I have not the slightest hesitation in answering in the affirmative. I know of no cases, where the opportunity has been afforded me (because the circumstance of being merely shewn a tumour for a

* See "Medicine and Surgery One Inductive Science."

few moments, and asked an opinion as to its nature, is no opportunity at all), wherein the coexistence of disturbed function, deranged organs, or the persistent operation of notoriously efficient causes of such disturbance, have not been either absolutely demonstrable, or inferred with the highest probability, in harmony with the invariably admitted operation of such causes; and that, in cases amongst the least promising of all, both the presence and the nature of the disorder have been further confirmed, not only as to its abstract existence, but also in regard to its relation to the tumour, by the absorption of the diseases contemporaneously with the gradual correction of the alleged disorder. Further, that the reasoning has been often kept as clear as possible, both in cases of total removal, as well as in various degrees short of complete success, by the avoidance of any local application which could tend to obscure the real causes of the result. As the detailed description of different kinds of tumours forms no part of my *present* plan, I shall sum up, in the conclusion, the general results of my experience to this period; having explained, as well as I can, the kind of investigation I make, and the manner in which I use the facts which it may unfold.

Such investigation I feel to be very imperfect, but still it is an advance on any other with which I am acquainted. I have no idea that any thing less will avail us; but whoever will pursue it, will soon, I think, be convinced that it is capable, by very obvious extension, of increasing our knowledge not only of tumours (for it is peculiar to no one disease), but of diseases in general. It leaves no doubt on my mind as to the *general* causes of tumours; that they are

the result of disordered function, and can only be got rid of permanently by its correction. How far we shall be able to determine more *particular* relations of these diseases by their proximate or ultimate principles as albuminous, fibrous, carbonaceous, azotic, and so on; or their special relations to organs as being of hepatic, splenic, or renal origin, &c. time only can show. At present, with some little reservation, which I shall by and by mention, I have not been able so far to individualise any one form of tumour, as to trace it exclusively to the disorder of any one organ. In the same kind of tumours, different organs have appeared to me in different cases to have been primarily or chiefly deranged. At present this is a subject of intense difficulty, on account of the time, labour, opportunity, chemical assistance, and many other requisitions. It is certain, however, that if, by the requisite vigilance, almost every tumour, of whatever kind, may be observed on certain occasions to be quiet, unannoying, stationary, or less (by measurement) than at others,—that such tumours exemplify conditions and changes, the practical imitation or establishment of which would either render the tumour of no annoyance, or cause its absorption,—and if we should find, as I believe we shall, that the relations between disordered organs and unnatural depositions furnish a practical exposition of the causes of the latter, and point out the only means of relief,—we shall scarcely do more than is done already in respect to gout; where we have unnatural deposition—characteristic disturbance of organs—the acknowledged influence of persistent but *remotely* disturbing causes—severe exemplifica-

tion of the inefficiency of mere medical treatment—a crowning proof of the superiority of that practice which rests *chiefly* on the avoidance of disturbing causes—the necessity of adapting the agenda of organs to their power—and, lastly (organic disease apart, and a persistent observance of the required cautions), successful results. I will now, then, proceed to consider generally—1st. the distinctions of health and disease; 2nd. the mode in which we shall most auspiciously investigate the seat and nature of the latter; 3rd. the manner in which I have endeavoured at its correction; and, lastly, a brief allusion to such results as I have obtained in the class of diseases under consideration.

CAP. III.

OF HEALTH AND DISEASE, AND OF CONDITIONS OF TRANSITION FROM ONE TO THE OTHER.

Few things have more impeded the progress of surgery than the want of definite and clear ideas of the distinction of health from disease; and of that disordered condition of the body, which for the sake of simplicity rather than be cause it is a strictly correct form of expression, I term the state of transition from the one to the other—a state, the detection of which is of the greatest importance, for on this will often depend, not only the discovery of the causes and nature of disease, but also the perception of the means by which it is to be relieved.

I hear continually both patients and medical men say, "The general health is good"—"There seems nothing wrong in the general health"—"All the functions seem properly performed," &c. in cases where nothing is more easy than to demonstrate the contrary. Now, in morbid anatomy, an important truly, but still a comparatively very small, branch of enquiry, no man thinks himself competent even to study, much less to apply it, without informing himself thoroughly as to the marks which distinguish healthy structure; and if a system of

surgery is to be recommended which is professedly based, no matter what the disease is, on correcting the functions of various organs which may be faulty, the whole matter turns on our distinguishing what is faulty from that which is not so.

For if a surgeon of that kind find no organ wrong, why his power is exhausted, unless he at once rush into a wild empiricism, which he will be a very unlikely person to do. Now the determination of the signs of health are not so simple as many persons may imagine: we will, therefore, take a wild animal, I do not mean hares, rabbits, or other game, but a really wild animal; which in this country, perhaps, cannot be done, strictly speaking, but which may be represented sufficiently for our purpose by birds or other animals of prey, hawks, weasels, stoats, &c. and note down the characteristics. We shall find, in almost every case, the—

1. Skin clean, pliable, inodorous, or inoffensive; or, if otherwise, characteristic of the animal, as in foxes, polecats.

2. Teeth regular, white, and sound.

3. Breath always inoffensive, or characteristic of the animal.

4. Tongue moist, vascular, and clean.

5. Fat, little or none, unless some special reason for it, presently to be remarked on.

6. Muscles very firm, and the degree in different muscles in direct ratio with their activity; so that, if you know the habits of an eatable animal, you can always tell, without being an epicure, which part will be most tender.

7. Habits active, excellent wind and speed: exhibiting on many occasions proofs that they possess,

in regard to almost any amount of mutilation, not involving the internal organs, most astonishing reparative power.

8. Excretions characteristic, so that the haunts of the animal can be recognized by them.

9. Colour of skin, and substance by which it is covered; and, consequently, *relation of both to heat* remarkably uniform.

10. Disease extremely rare, and, in really wild animals, unknown.

We have been selecting animals as wild as we can get within our ordinary observation, that is, for whom no express provision is made by man; and who, were it not for some accidental assistance, would have none but what nature provided; who have natural food, natural habits, natural activity, natural enemies, &c.

Let us now make a step towards more artificial life, so far at least as regards comparative freedom from molestation, greater facilities of finding food, but still unlimited range of space; in fact, into a country where game are preserved; we still find many of the attributes of which I have spoken; but we get two things which cannot be disputed, a good deal of fat and a great deal of disease. We also begin to see more frequent varieties in colour, as in partridges, pheasants, hares, rabbits, deer, &c. In the development of disease, in great numbers we generally observe that the influence of unfavourable seasons is an immediate precursor. That the vicissitude of season is really not the new circumstance, but that the thing unobserved before in the wilder animal is *the incapability of resisting it.* Now if, stepping over some gradations, we at once proceed

to view animals in a domesticated state, we shall observe that many of the attributes, with which we set out, are either extremely inconstant, or altogether wanting.

We often find the breath that was inodorous or inoffensive very much the contrary, the skin dirty, teeth more or less yellow, the tongue neither always clean nor always moist; plenty of fat, or great leanness; and diseases have become so common, that hundreds of people gain their living by professing to cure them. Further, that when any of these domesticated animals are of great value, that we labour at an immense expense to restore the conditions we set out by remarking; especially as to activity, cleanliness, condition of skin, reduction of fat, &c.; and that we succeed, cæteris paribus, in the objects with which we do all this, in proportion as we can restore such conditions; as in hounds, race-horses, &c.

Now, then, instead of pursuing the various links represented by the different *classes* of domesticated animals, all extremely instructive, even to a lady's lap-dog, let us go at once to man, and to civilized man as he is called, and see how many of the conditions he presents which we observed in the wild animal. Where is the clean tongue? where the inodorous breath, the clean teeth without dentifrice? where the clean inodorous skin, the wind and speed, the fine, almost fatless, clearly depicted muscle, the power of repair without surgical aid, &c.? Do we not find the tongue seldom moist and clean—the breath, if not always offensive, still presenting all sorts of odours—the teeth requiring daily cleaning —the most reasonable exercise too often inducing

fatigue—speed disturbing the respiratory and circulating organs? Repair of wounds and other injuries, if severe, generally tedious, often uncertain, always requiring care; and even the most trivial, sometimes fatal. In short, from the moment that we begin to interfere with the natural habit of animals, we observe the beginnings of disease, and pari passu, as the animal has become more and more domesticated, diseases have rapidly multiplied, until they have been brought out, as it were, into that frightful relief which is exemplified only in man. Now we have not only hundreds of men devoting their time to the study and relief of diseases, but the enormous catalogue has suggested various motives for the division of labour: so that we have physicians, surgeons, apothecaries, aurists, oculists, ladies' doctors, children's doctors, doctors for particular complaints, doctors of particular systems, homœopathists, hydropathists, and mesmeric doctors. In short, all sorts of men, all sorts of systems, in all sorts of combinations, striving and striving in vain to crush the hydra that is represented by the many-headed catalogue of diseases. Now, although, besides an amount of misery, of which few have the slightest idea, all this likewise involves an incalculable amount of moral degradation,—and although *so large a portion* of the evil may be shown to arise either from the purest and most easily informed ignorance, or from causes absolutely and easily remediable,—yet scarcely an effort has been made on the part of our governments to diffuse, nor, till within a few years, even to seek, information as to the causes in question. And so slowly do matters proceed, that, notwithstanding the Report to the Committee

of the House of Commons, and Mr. Chadwick's most acceptable book, both excellent digests, and reiterations of what every surgeon of a large dispensary in London must have known and lamented for years; still a Bill for the better regulation of drains, in other words, for the restriction of the diffusion of virulent poisons, proposed about four years ago by Lord Normanby (to his honor be it spoken), has not yet made its way through the House of Commons. Even those of the profession who are contesting for changes and improvements of our medical polity, as well as those who still cling to things as they are, seem, on an analysis of their respective proceedings, to be much more attentive to the pecuniary relations of their respective plans than they are to the improvement of science in the abstract, or its diffusion and application to improving the physical, and through this exalting the moral, condition of mankind.

Let us now consider whether the phenomena which we set out by noting in the wild animal be as characteristic of health as they were evidently accompaniments thereto. Why, already we have, in the mere collection of the facts, gone far to prove that they are; for we have shown that, as the phenomena in question disappear, diseases are developed, and that when they are *all* gone, diseases appear in countless forms, numbers, and combination: nay, more; we have obtained in some sort a crucial experiment; for we have observed that if we are to restore the more striking of these conditions, we are obliged to restore, so far as we can, the *natural habits* of the animal; or, what is more immediately in point to this stage of the

argument, habits that are *notoriously productive* of increased power; as in the hound, race horse, hunter, &c. But it may be said, "after all, you only prove what is already admitted; namely, that natural habits, activity, and temperance are the elements of health, and the opposite of these the parents of disease. Of this mankind are aware; but they will not act in conformity with the principles it discloses." Now, although the whole of this is not strictly true, since the bulk of mankind are in a most profound state of ignorance on the subject, still I admit, that had we nothing more to urge, and could we not educe from the foregoing something more particular in regard to the practical management of disease, we should have so far laboured in vain. But the fact is far otherwise.

If we subject the condition presented by the two ends of the chain which we have been rapidly tracing, viz. the point at which we set out by observing a wild animal, and that at which it terminated in the man, to a further analysis, we not only obtain a demonstration that the phenomena first noted were really signs of health, but we get evidence of other very important conditions; conditions not at first so readily observable; and which teach us, amongst many others, three things that are especially important. 1st. That the state in which all the alleged signs of health are absent is in civilized society (as it is called), man's ordinary condition; one in which the functions of the œconomy are performed, it may be; but one in which they are not *distributed naturally* to the respective organs: that, in fact, it is a state which is certainly not health, and which, as it is not a state expressed by

the word disease in its ordinary acceptation, but is in fact a condition which invariably leads thereto, so I simply call it a "State of Transition." Secondly, the analysis furnishes us with a very good general idea of the more essential and important characters of such a state of transition. Thirdly, it enables us to obtain very considerable insight into the mode by which nature keeps off disease, and (in a number and variety of adaptations of an exquisitely beautiful law) unfolds to us the most rational method of relieving it; and, what is, if possible, a still higher object, of preventing its recurrence.

Now, then, let us take any man we meet, and, for illustration of the rule, let us take one engaged in the ordinary pursuits and habits of mankind; we now wish to know whether this State of Transition is the rule; and whether it be *truly designated* as one in which the duty imposed on some one or other organ is unnatural. That is, whether observation of the various organs can elicit that one or more of them, either from executing the duty of some other organ, or from other increase of its own function, are bona fide doing more than in perfect health they are designed to do; and whether the evidence is or is not so clear as even to admit of proof from the contrast afforded, not by a wild animal, but by such exceptions as are presented by the more healthy examples of man himself. Now what function shall we examine? it matters not, for in different cases all bring their evidence in turn.

One of the most obvious is the exhalation by the mouth (the breath). It should be composed of carbonic acid, oxygen, nitrogen, watery vapour, and the exhalations from mucous membranes, all of

which are nearly inodorous. But how often is the breath acid and peculiar, not to say offensive? how seldom is it, honestly speaking, otherwise? Then how often do its peculiarities, of themselves, suggest some extraordinary exhalation. I knew a man, who was notorious for his offensive breath, who never scarcely knew what sickness was until just before he died. He never was a patient of mine; but I know that his alimentary canal was irregular in its functions, that his skin was inactive, and that his breath was exhaling some form of matter which smelt like many exhalations from the skin, only much more offensive*.

Another man will tell you (I am not speaking of patients), "I was never better in my life;" and yet at another time he will say, if the conversation lead to it, "Ah, the liver, that is my weak point; my liver never goes right;" and if you enquire of him, you will find, sure enough, that he has at least indications, as torpid bowels, characteristic complexion, &c. that he probably speaks truly; but now, if your curiosity impel you to proceed, you will find that his skin is very profuse, or his kidney secreting urine, loaded as to quality, liberal as to quantity, or both, or some other evidence of increased exertion *elsewhere.*

Some persons will say to you, "Oh, I hardly ever perspire;" but if you enquire into the various functions, you will find that either the pulse is frequent and the respiration also, or that the kidney is active; if you should meet with a case wherein you can obtain *no* evidence of this kind, you will find that the *heat-making* processes of the œconomy are

* He died of suppuration of the lungs.

inactive; that is, you will have the indications of a languid digestion, a slow, weak pulse, or both. What I mean to urge is, that the functions of the body must be performed, that disorder, in its ordinary and cognizable forms, cannot be absent for a day, unless many forms of matter be *separated* from the body; and though we cannot say, in regard to *every* function, how long it may be suspended without disease, yet no function can be really so suspended for more than a very short time, perhaps a few hours; and that the explanation of such *apparent* suspension is the institution of compensating actions; which in my experience are always discoverable; that is, where disease is yet *absent;* for be it recollected it is the co-existence of health, or *apparent health*, with *defective function*, that I am considering.

What I have already observed of the organs mentioned might be further illustrated by the liver and kidney, or indeed by any two organs, if I were to use the largest induction of facts which experience on the subject is capable of supplying; and which in the end would lead us up to the phenomena of what we call vicarious secretion, or excretion, in which one organ is demonstrably performing a duty usually assigned to another; as when the menstrual fluid has been secreted from ulcers or from the Mammæ, or the still rarer case of urinous products being exhaled from the skin. These phenomena, traced through the links which connect them with others more common, are really nothing more than exaggerated forms of those compensating actions which I am considering; and demonstrate the provision, represented by one organ having the

power to assist another, to be universal; so that there is no proof or illustration of it which might be demanded, however far-fetched or bizarre, but which peculiar cases occasionally furnish.

But we now arrive at the application of all this to the explanation of disease; and here it is that we obtain the desired fruction—here we obtain suggestions that we allow no longer to rest on the cold thankless basis of tediously deduced facts and arguments, but the truth of which we can test by the control which they exert over disease, and which triumphantly exemplify that even a measured obedience to the rules of an inductive or common-sense mode of proceeding is never more luminously successful than at the bedside of a patient. I do not scruple to assert that, in cases wherein all the most active routine has been uselessly, or worse than uselessly, employed, and where patients and medical attendants and friends were alike desponding and at fault, inductive reasoning, without any thing to work it but common sense, has educed in a few minutes the primarily affected organ, and the resulting measures have been rapidly successful. To proceed then: now, notwithstanding that the organs are daily executing their compensating functions, yet to this, as to every other vital process, there is a limit; no organ can *entirely* perform the duty of any other, nor a part of it, for an indefinite period: the result is always disorder or disease. Those compensating actions, which, in their *natural* and *completely* healthy manifestation, are employed only in conferring that beautiful power of accommodation to temporary varieties of impression on the œconomy—varieties incidental to change of climate and other

natural causes—are, in civilized society, and in the "State of Transition," as I have called it, employed in disposing of injurious influences of *more persistent character;* and, as a clear understanding of this difference, is not only of the first importance in regard to the subject we are at present considering, but, as I may hereafter shew, has important bearings in conducting us to the true nature of fever and many other affections, so I will, at the risk of being tedious, endeavour to express what I mean by contrasting the two conditions.

Any two organs will serve our purpose; but, perhaps, the skin and kidney will illustrate the matter most easily. Now, the secretion of the kidney is more liberal in winter than in summer; for, guard the skin as we may, the Animal feels the difference; and the rule being—that the surface of the skin is subjected to a somewhat lower temperature, there is less exhalation going off from it; and as, in animals whose supply of food is constant through the year, the assimilating and other functions go on, there is more excreted by the kidney. But, even in winter, this increased action of the kidney is seldom persistent. Exercise instantly, to a certain extent, restores the equilibrium. In summer, on the contrary, the skin is more active; its immense surface is employed in cooling the body, in exhaling heat, watery vapour, carbonic, acetic acid, &c. The kidney excretes less. Here again, however, there is a constant alternation of activity and *comparative* repose; for whenever we are at rest, and lighten our clothing in conformity with the suggestions of our feelings, the secretion from the kidney becomes more liberal. So that the *general* relation of these

functions in winter and summer being that which I have stated, still we perceive that there is nothing strained, persistent, or excessive. The influences requiring *these* compensating interchanges of function, are really natural changes; and I need not say that the natural provision for them is perfect, and its exercise *consistent* with the longest life, and freedom from disease—however rare the example. Let us now take a common case, one of the thousands by which we are surrounded, illustrating the more *persistent* effects of habitually imperfect action at the surface; let us suppose the injurious influence represented merely by the absence of exercise (sedentary habits), the assimilating organs being excited by food and drink in the ordinary manner. Now, unless the effects of active assimilation, with sedentary habits, be adjusted by occasional indigestion, there is increase of bulk—a very common, but by no means a *necessary* consequence; whereas it is absolutely so, that the skin and kidney should, somehow or other, obtain the separation of certain things from the body.

Now, here the causes of excitation of the kidney are persistent; the skin is *minus* its natural excitement, exercise; the condition of the kidney is far otherwise. The mere circumstance of the skin doing little, stimulates the kidney to do more; as is natural: but, very soon, additional requisitions are imposed on this wonderful organ; soon, sedentary habits affect digestion, the assimilation is seldom perfect, and of this the urine is sufficiently demonstrative. Here, then, the compensating activity of function is permanent; that is, the kidney *habitually* secretes more, the skin *habitually* secretes

less; and we know how many years this condition will go on, attended, it may be, with fits of indisposition only; but are not these the very people in whom we should, consistently with these views, find—and are not they, in fact, the persons in whom we do find—examples of the most afflicting maladies. The kidney obliged to constant and excessive labour; to separate matters which exhaust its energy, and disturb the securities afforded by the solvent power which its exquisite chemistry affords; the urine becomes loaded with more than it can hold in solution; and hence deposits occur, constituting the beginning of those evils represented by the dark catalogue of calculous and many other diseases of the urinary organs.

The reader will, of course, not understand me as saying that urinary diseases are all caused in this manner. I have merely taken one series of facts represented by the skin and kidney, as familiarly shewing, first, how a comparatively little dreaded injurious influence will certainly lead to mischief, if it be persistent; and as illustrating the manner in which a *continued* obligation on an organ to do more than is in health naturally assigned to it, explains the dependance of practical comfort, ordinarily called health, on a state of things which are nevertheless the true precursors of disease; and as impressing the fact, that the essential character of health, and consequently the *final* desideratum in removal of disease, is the apportioning to each organ its *due* share of duty; in which, doubtless, is included those *occasional* and *temporary* augmentations which I began by first exemplifying. At this stage of the investigation, we proceed to consider disease, *com-*

monly so called: and now we see the use of the preceding enquiry, of which I have been enabled to give only an imperfect sketch*.

The very first fact we perceive, when disease, *commonly* so called, does occur, is, that it is not seated in the organ to whose functions the disturbing causes were originally applied, but to that which had been employed in the *compensating function*. In the whole range of physiological enquiry, there is no point, in my opinion, comparable in practical importance to this; because, in nine cases out of ten, it produces this startling result, that not only are the mere *symptoms* of disease very indifferent evidences of the *real* state of the *various* organs, but that the seat even of what we call *disease* is very seldom the seat of the *cause* of the disease. In other words, the seat of the disease and the seat of its cause are seldom the same. The want of the knowledge of this fact seems to me to be at the root of most of our errors; and the evils of our deficient modes of enquiry are never brought out into the required relief, until we arrive at the perception of this fact. *Our* alleged causes are almost always effects. Let any man only look at the history of the disease called diabetes, and see if the progress of correct views have not proceeded exactly in proportion as the seat of the disease (as alleged) has been perceived *not* to be the seat of the cause. How many kidneys have been examined with such unsatisfactory results; as well indeed they

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* Should he desire it, the reader will find the subject of "compensating functions" somewhat further considered in my "Medicine and Surgery," p. 190.

might, when, in many of the so-called diseases of this important organ (diabetes especially), it would have been much nearer the truth to have regarded the kidney as the only organ that was sound; cases in which this great sewer of the body had only been worn out by having to drain matters which ought *never to have been thrown into* it; that is, when (as Dr. Prout and others, as I think, have clearly proved) an imperfect action of the stomach, and other assimilating organs, have imposed upon the kidney the reduction of sugar and other products, which form no part of its natural and healthy duty.

I have elsewhere shewn the importance of attending to the phenomena of sympathy; and every one knows how important Mr. Hunter thought them. In Mr. Hunter's work, you will find a remark, which is in fact a sort of hint, in regard to the proposition I have been enunciating. He says, "Sometimes the sympathy in the organ sympathizing will be more prominent than the affection of the organ with which it sympathises," &c. Of this, of course, there are many examples; the stomach, for instance, in many uterine conditions. Now the important principle that I have been insisting on, is nothing more than a similar condition; expressed by the fact—that a compensating organ has been so long doing the duty of another organ, that it has at length become diseased. There, then, are the symptoms; then how, it might be said, can we get at the cause, since you say the symptoms are referred to the compensating organ; in other words, to one secondarily affected; in other words, in fact, to

effects*. I answer, that *generally* "nothing is more easy." That you have only to extend the application of principles with which you are already familiar, and of which you daily acknowledge and prove the value.

You have only to distrust a *mere* symptomatology; and to enquire not only as to what organs *display* by altered sensation or otherwise; but into all *causes* of disturbance to which they *have at any time* been subjected, which your knowledge of the animal œconomy enables you to recognize.

If you know that a man is a dram-drinker, even though you do not ask him a question, do you not at once conclude that he has probably diseased liver? nay, do you not form a similar prognosis when about to examine a dead body, and perhaps which you now see for the first time? and are you not generally right in both cases, if the opportunity of ascertaining the proof be afforded you? In the one case, death was the only *symptom;* in the other, symptoms might have been present, but the man was not your patient, and you were not called on to enquire into them; but the universal use or abuse of alcohol has furnished you with abundant proof that it is an injurious influence, and, whatever else it may do, that frequently it acts injuriously on the liver. On this, without any symptom, you have prognosticated, and you have prognosticated truly; and I can assure you that in *many* such cases, though you had examined for symptoms, you would have found

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* That this is the *rule* in regard to what we call disease, is, I think, demonstrable beyond the possibility of dispute. But it is not without exception. I could mention many examples where the symptoms are referred to the primarily affected organ.

none of diseased liver. I say in *many* cases considerately, and in an absolute sense, and not of course relatively to the thousands in which there *are* symptoms. Again, even when you *have* symptoms in the dram-drinker, and you have seen him stand shattered and tottering at the door of a public-house, and coughing and expectorating, and showing symptoms of hepatized or consolidated lungs, of his bronchial surfaces being in a state of labouring activity; or of diseased heart, &c. still you continue to look to his liver, as the portal through which the poisonous agent has principally wrought all this mischief, and speak of it accordingly.

Now, if, in the investigation which I would recommend you, for alcohol you read injurious influences of all kinds whatsoever with which you may be acquainted; and, if for liver only, you will read all the organs of the body; and the nervous system, as a whole, as affecting or affected by these organs, and the mind also; you will make just that investigation which I am beseeching you to make, and which will in most cases conduct you to the discovery of the primarily affected organ. Because, although it seems a great deal to say, the truth is, that it is not because our information is deficient, that we cannot apply it to the elucidation of the causes of disease; so much as that we do not employ it properly.

If we *will* judge from a portion only of the facts on any subject, how can we help being deceived. If a chemist were to proceed in that way, he would obtain nothing but error; he would get a great deal more *useful* truth in simply examining a body by his external senses, than he would by inferring

its composition from the analysis of only one of its principles. Unquestionably, in all investigations, the tact and experience of the investigator will influence the results, because the *mode* of asking a question of nature does not always imply that faculty which suggests the question. Still nothing is more true than that the enquiry I seek to recommend is remarkably simple; it requires in the main no knowledge but what every tolerably educated medical man has at command, and nothing which is not very intelligible to a patient; in my opinion, one of the greatest points of all; for he alone can enable us to carry out the demonstration.

In collecting the facts of a case, of course the induction should be as comprehensive as possible; because if we allow any fact to escape us, we are never sure, in regard to any subject whatever, that it may not vitiate the whole of our reasoning. In order to facilitate the making of this enquiry, and registering the main facts which it may elicit, I am in the habit of recommending the following Table of Record. It suggests the principal points of enquiry to the young investigator, and is extremely serviceable to any body in the unravelling of difficult cases. To those who investigate after this manner for the first time, I would recommend a frequent use of it, even in cases which they may not think sufficiently serious or difficult to require it; since it is of itself a good exercise; it arranges the facts conveniently for study, and practice soon enables you so far to dispense with it as to confine its use to cases of unusual duration, interest, or difficulty. I have here given the heads in a small space. In use, they are arranged on a large folio sheet, one leaf

being occupied on both sides by the table, the fly-leaf being left for observations. I hope the time may come when the facts suggested by such a table may be regarded as a very limited collection ; at present, I should fear to recommend any enlargement of it. I will now give the table, and then make such observations on some of its points as I think may be useful to my younger brethren or to the public, and which I may be able to include within the limits which I have prescribed myself.

TABLE OF RECORD.

Name, Date, Age, Occupation, present and former.

Born, Residing.

Soil, Sewerage, or other Local Peculiarity.

Climate.

Mode of Life.

Moral Causes, Diseases, Accidents.

Treatment.

Brothers and Sisters.

Other Relations in Blood.

Hereditary Influences.

Former Life as ascertainable.

Summary of Injurious Influences as deducible from foregoing column.

Temperament as Apparent.

Disease or Accident.

Alleged Duration and Progress.

Anomalous Symptoms.

Nervous System, Brain.

External Senses.

Medulla Spinalis.

Assimilating Organs, Tongue, Stomach.

Liver and Pancreas.

Spleen.

Bowels.

Respiratory Organs.

Circulating Organs.

Excretory or other Organs, Skin.

Urinary Organs, Kidney.

Bladder, Urethra.

Genital Organs.

Results of Chemical Analysis or Tests, if any.

Summary of Symptoms, &c. when deemed convenient for reference.

Treatment, with what changes of Function, if any. Diet.

Exercise.

Air, Temperature, Direction as to.

Medical or other additional Treatment and Observations.

Remarks on the Table of Record.

In my remarks on the different compartments, I must necessarily be brief, because a large volume would scarcely contain the exemplifications of the necessity there is for investigating the points to which they refer. I would beg the reader to observe that there is not one of them which involves any peculiarity of opinion, or which supposes any influence in regard to disease, but such as the whole world acknowledges, at one time or another, to exert an agency in its production.

The frequent importance of age is too obvious to require remark; nothing, however, impresses more strongly than the whole investigation, how *relative* the effects of mere time are, as opposed to the *absolute* effects of injurious influences.

The nature of the occupation in which a patient has been engaged, is, in many cases, very material. During the twenty years that I held the largest dispensary in London, situated in a neighbourhood occupied by all sorts of mechanics, the influence of various kinds of employments in disease would necessarily be strongly impressed on me. Familiar with the injurious influence of particular employments, I have often been enabled rapidly to relieve disease by the simple act of visiting the patient at home, when I have observed that, although the medicine apparently acted as I desired, and achieved a certain degree of amendment, still the case did not progress to recovery. In many such a case, the home visit has at once unravelled the mystery; and

the discovery and avoidance of the injurious influence has rapidly effected the cure.

I could, however, do little towards the development of this important subject, without writing on the diseases of particular occupations; but, as I am anxious to impress the fact, I will select the "water-gilder's," as one amongst many others which, whilst it is a glaring exemplification of the influence of particular occupations, shews how little idea people sometimes have of the misery which may result from the simple act of covering a metallic surface with gold. In gilding the works or the case of a watch, the metal is first covered by an amalgam of gold and mercury (I believe in *nitro*-muriatic acid); the article so treated is afterwards held over a charcoal fire; the heat sublimes the mercury, but leaves the gold; and as this takes place, the surface is constantly rubbed, until a fine polish is produced: since I first saw the process, now many years since, the charcoal fire is partly covered by glass, which must lessen the mischief; but still the operator must breathe, in a greater or less degree, the fumes of carbonic acid and mercury. What could you expect to do in such a case by medicine!!! This is, perhaps, a more striking illustration than many others; and I have seen many a wretched victim of this occupation; but I question much whether it is a whit more destructive than many others—which are even more dangerous, as being more insidious.

Many persons, who have entire command of their time, institute influences, under the force of habit or the suggestions of inclination, quite as mischievous as those to which the water-gilder is subjected; and which destroy as certainly, though, perhaps,

not always so soon. I have known sedentary habits, with what *would be called* avoidance of all intemperance, produce as fatal and as decidedly a "gin-drinking liver" as ever resulted from the abuse of alcohol; and no poison can do more. The fact is, that in the two extreme cases which I am taking to illustrate the importance of habits on the state of organs—viz. the application of a direct poison on the one hand, and the simple want of exercise on the other,—the difference is just this, that in the one case the Œconomy has to deal with an influence to which it ought not to have been subjected; in the other, a natural requisition is withheld from it. Small doses, if I may so speak, of injurious habits *administered daily*, are, after all the slow undermining, unperceived, elements of mischief.

Occasional *excess*, with a really *habitual* temperance, does not do half the harm that that apparently innoxious laissez faire sort of living does, which so commonly characterizes the habits of the less occupied and wealthier classes. Both, of course, are bad; but, under the one disturbance, nature is clamorous and unequivocal; in the other, she is daily employing resources, which, from their very beauty and perfection, alike lull vigilance and suspicion.

Although I must hasten to other points, I cannot leave this section of the subject without a few words in regard to alcohol. We are accustomed to connect the abuse of this fluid with either the lower, or with a particular class of persons in the middling and upper grades of society; such as the more dissipated of our young, or the more indulgent of the old. But, unhappily, no rank, nor either sex, is so free from the influence of this poi-

son, but that we occasionally find examples of its mischief every where; and sometimes, where the habit is most mischievous, it is extremely difficult of detection. The practitioner should recollect that alcohol is an ingredient in many things, besides wine or beer, or what are ordinarily called spirits; liqueurs, eau de Cologne, tincture of bark, or cardamoms, and many other things, have been made thebearers of this pernicious influence; and having thus said enough, I hope, to awaken vigilance and impress discretion, I shall dismiss the subject.

Relations between the native climate of your patient, and that in which he may have resided, or is now living, are often of great consequence; not only as to the general influence of vicissitudes of temperature, &c. but as suggesting enquiry as to the relative activity of different organs, at the period of transition, or subsequently thereto.

Thus, I have found a man habitually costive who had lived a good deal in warm climates; and, in a very few questions, got the key to his complaint—at all events to the cure of it—without medicine, by restoring activity of the skin. Many similar examples might be adduced in regard to other organs.

For many years, I used to be almost laughed at, for laying *so* much stress on drainage and soil, although I never contended for any thing but the correction or avoidance of such as were acknowledged to be injurious; and, even now, I occasionally meet with almost incredible examples of indifference to this subject, in the wealthy and otherwise well-informed; but the Report, before

alluded to, of Mr. Chadwick, will, I hope, in time produce the required impression.

One of the points, however, of which my experience on this subject has informed me, has a very interesting, because a very general, bearing on the *nature* of disease, as modified by the state of the body; viz. that in a case where bad drainage shall have been demonstrably the cause of many persons being ill in the same house, they may all be affected in a different manner, or, as we should say in common parlance, by different diseases.

The whole of that section of the table which refers to "former life," I hold to be more frequently than any thing else the key to the really difficult and obscure cases. I have already alluded to the disease of organs having been in this way inferred from the operation of former and more or less persistent influences, where the most careful examinations could detect no *symptoms*. Of "moral causes," I may observe, that the whole subject is most important. I have only room for a few practical hints. Their operation is very often, perhaps generally, obvious; but it is interesting to know that in many cases they are so successfully concealed as to lull suspicion. In going through a case where I expected difficulty, I have been answered every question with a ready, cheerful, even crisp manner, much more like the cheerful hilarious expression of health, than suggestive of moral disturbance; when, on enquiry as to whether there had been any cause of peculiar distress or unhappiness, the eye has become suddenly suffused with tears and the voice choked with emotion. Yes, nothing is more true than that the

countenance, often so useful an indication of condition,—one that will often tell truth to the searching eye of a medical man in spite of the wearer, will, nevertheless, sometimes express peace when there is no peace,—when, in fact, there is a worm that never dies,—when the mind is haunted by some depressing and constant source of inquietude. Here there is much that depends on the judgment of the practitioner: for whilst the general impression it should convey is a discreet caution as to the use of medicine—seeing that medicine cannot, in a direct sense, "minister to a mind diseased,"—yet, on the other hand, it is essential to remember that nothing more indefinitely diminishes the force of mental disturbance than the judicious regulation of the various functions of the body. We cannot deny that medicine, circumspectly and cautiously administered, represents *one* of the means which contribute to this object; and the adjustment of its application, in such cases, or its entire avoidance, forms, in my opinion, one of the highest functions of a medical man.

Before I conclude my remarks on the section of "former life," I would observe that it has been chiefly to inattention to the enquiries it suggests that I ascribe certain differences of opinion with men for whose talents and judgment I have a high respect.

That former influences affect the state of the body, no one doubts; and if a man *will* form his opinion on a case, on half the number of facts on which you think it necessary to found yours, it is absurd to talk of his talent or his reputation: they weigh nothing in such a case with any man who has

the smallest confidence in himself. I recollect once seeing, with a distinguished physician, some peculiar results from a neglect of this kind; and when I stated, amongst other indications, that the patient had had severe jaundice,—"Oh, yes," he said, "but that was seven years ago." Truly it was; but it was not less important as thickening the evidence in regard to the liver.

Hereditary influences are too generally acknowledged to require remark. They are often extremely instructive; but no one will educe all the assistance from this branch of enquiry which it is capable of yielding, unless he proceeds on the impression that *acquired* peculiarities, the result of bad habits of mind or body in the parent, are as truly subject to transmission as gout, or any of those whose *real* origin is concealed under the somewhat loose application of the words *constitutional* or *natural*. *The "summary of injurious influences,"* if deduced with due caution, will be found convenient, as saving time when we have occasion to refer to such portions of the first column.

With regard to the *history and symptoms*, I would say, let patients first tell their own story; they may often relate many things that you do not require to know; but, if not interrupted, they will often communicate a very capital fact which all your questions may fail to elicit.

Those who think the method I advise tedious, have, I suspect, little practice in severe cases or rigid modes of enquiry. It is by no means easy to extract the whole facts of a case: the avoidance of leading questions; the application of previous knowledge and tact in adjusting, not only the value,

but even the reliance to be placed in the various answers to our enquiries; all require considerable discretion.

In law, the examination of evidence is regarded as requiring great ability; in extracting that whence you are to educe the state of an animal body, you have all the difficulties of the lawyer, with many superadded. You have not only often a very unwilling witness—not only one who is often very ignorant, but one, also, who is very forgetful; one who has, perhaps, succeeded in deceiving himself, and, therefore, who may *even unconsciously* deceive you.

The increasing facility which the habit of analytical enquiry confers, soon teaches us how many things there are required in a medical examiner; and although there is nothing difficult to those who go to work with the required care and freedom from prejudice, yet it is often remarkable how inefficient those are who investigate for the first time *under the influence* of mere routine modes of enquiry.

I can assure the *student* that nothing is necessary but patience and common sense; that the extra time occupied is generally confined to the first, or one or two subsequent visits; that, however satisfied a certain class of patients may be with a hasty visit and a prescription—which fashionable models may have induced them to regard as the type of tact and ability,—the mass of society are beginning to awake in regard to this subject; and that it is my firm conviction, other things being alike, that he who commences a difficult case by a thorough examination of it, will (if he have but health) not only reap the largest fruition, as regards the

extent of his practice, but the soundness of his reputation—two very distinct things.

In respect to the mode of determining what organs are at fault, and to which our measures should be primarily directed, it is necessary to recur to the proposition that the "causes of disease and the seat of disease" (as that term is generally used) are not the same; that the peculiar actions in the organ, or part, though the immediate cause of the outward sign or symptom, are themselves effects. What I mean is simply this: the cause of diabetes is not in the kidney; the cause of cutaneous diseases is not in the skin; the cause of tumours is not in the tumour; and so on. With regard to the digestive organs, commonly so called, the cause is sometimes in the organ in which the symptom is, sometimes not. That is, sickness, loss of appetite, indigestion, may be derived from conditions of the bowels or the liver, &c.; from causes acting directly on the stomach, or through some other organ. The organ to which the symptoms are referred may be primarily or secondarily affected; and, as I have repeatedly urged, generally the *latter*.

As, according to the system I am advocating, the proper practice is to minister to the primary organ, its discovery is in all cases the chief desideratum, and in many the only difficulty:—whatever little knowledge I have obtained as to the various means by which we can educe and test the evidence of this important point, I could, of course, communicate; but it would involve so many details, that I could hardly do it with less opportunities than are afforded in lectures. I will, however, give such hints as my limits allow.

In a case, then, of tumour, or any other which you are about to examine, with a view to detect the primary organ, we will say your patient refers all his complaint to his stomach; but, although you are ready to admit that this organ is often primarily affected, the whole of the facts induce you to question such a view of the case: you find, for example, no evidence of any gross abuse of the stomach, the tongue presents nothing very strikingly bad, though, perhaps, not healthy; your enquiries fail to elicit, either from hereditary or other sources, any evidence which seems specially directed against the stomach; you have no *symptoms* pointing to the liver; but your patient is excitable, and reports his habits as sedentary; but he is fair, and presents no appearance of what people *call* a bilious temperament. Well, you can find nothing very prononcée. Are your means to be directed to the stomach, or whither? why, perhaps it is not very material, you think; because the means *specially* adapted to tranquillize the stomach will have a generally tranquillizing effect on other organs; but, from the reported inefficiency of former treatment directed to the stomach, or for some other reason, you suspect the liver; and, on inducing a flow of bile, you find instant relief; you then hear, perhaps for the first time, that the affection of the stomach had often supervened, on the use of articles which you know to be in some persons specially disturbing to the *liver*. You prohibit these; and your patient's stomach, which had previously resisted a great variety of treatment, remains free from annoyance.

Mutatis mutandis, this will happen with any two organs of the body. It is true, for reasons which

I explained when I first applied the phenomena of sympathy to the improvement of constitutional treatment of disease in the manner directed in the "Unity of the Body," that you very often obtain good effects by acting on secondarily affected organs; but the influence of this practice is neither certain nor uniform, although the perception of the principle, in enlarging the sphere, increases the utility of its application, by opening additional channels, through which you may endeavour to act on the organ you desire to influence; but if you detect the organ primarily affected (in acting on which the principle just adverted to is often an useful assistant), you obtain a certainty and uniformity of result as, I believe, is unequalled by any other mode of practice. Take another case of a different kind. A lady tells you that she has been a valetudinarian for years, and enumerates a host of symptoms, which, by turns, seem to point to every organ of the body. Her habits, *by her account*, involve nothing that is wrong; and, in short, with symptoms of half a dozen diseases, you are at length disposed to say that the case is hysteria, which to me is much the same thing as saying that you do not know *what it is.*

In a few minutes the patient mentions a symptom which your previous enquiry had not elicited; I mean copious leucorrhœa. You find that it is abundant and pretty constant; and that the discharge *generally* continues from one menstrual period to another; but she adds, "I have had that for years; I had at one time a great deal of advice for it; and I took bark, steel, and used injections, and it ceased for a time; but I did not fancy myself so

well for it," &c. This is a hint; you now sedulously enquire in what way she felt herself not so well, and at length obtain evidence which leads you at least to think that you have discovered the primary organ, which in the case before us we shall suppose to be the liver.

That you now try to test this by inducing action from the organ; but though you proceed cautiously, still your first two attempts act on the bowels. You alter your prescription, and the third time you obtain *moderate* action still from the bowels, but a very *copious* discharge of bile. Your patient, at the next visit, tells you, with great glee, that she feels much better; and when she has expressed her gratitude for her amendment, she adds, of this leucorrhœa, "that the weakness has entirely ceased." Only one week has elapsed since your last visit; you treat her as if the liver was the primary organ; she gets rid of her hysteria, and the leucorrhœal discharge does not return. Here is a discharge of years cured in a week by the correction of a latent disturbance of an important organ. The leucorrhœa was here, according to my views, the compensating function, for a conservative purpose, and which it fully effected, so far as the safety of the œconomy for a time was concerned (although at the expense of a tormenting state of system), notwithstanding a torpid liver. Something more will be said in regard to this class of case in the few remarks I shall offer on the *treatment* of the liver, to which I refer the reader.

In many cases of disordered health, where you cannot detect the primary, or really affected, organ, the facts of the last case afford you great assistance,

and render some of the most common cases in surgery inestimably valuable. E. G.—A man has an ulcer; he has had it before, and it has healed and "broken out again;" the common history of ulcers of the extremities. Believing it to depend on disturbance of some internal organ of the body, you desire to cure it by the correction of that function; but you cannot find what it is. Here the compensating action of the disease, that is the ulcer, keeps all quiet, and foils your enquiry. You *now omit all general remedies*, and try to heal the ulcer by strapping and bandage, watching the result. Your object is to elicit the defect for which the ulcer is the compensation; and you believe that if you can heal the ulcer by local means, the really affected organ may declare itself by some symptom, and thus guide you to the true mode of permanent relief. This is merely an artificial mode of ascertaining that of which the history of ulcers shows you that nature frequently informs us without treatment, viz. the healing of ulcers and the supervention of symptoms indicating disturbance of some internal organ. Every one knows, also, how frequently it has happened that the sudden healing of ulcers has been followed by fatal diseases; shewing us, by every kind of evidence, that those diseases, like tumours, represent new and special functions, set up to meet some exigency in other organs consequent on causes which are disturbing them in the execution of their duties. The habit of setting up compensating actions in the body is so constant before the appearance of what we call disease, that if we find an organ excreting any thing unusual, we may be in general certain that some other organ is proportionably defective. This

may, indeed, be one of the assimilating organs, usually so called, or an excreting organ; that is, the thing rejected is something which ought to have been assimilated, but is rejected on account of its being combined in some form which is hurtful to the œconomy; or it may be effete matter not ejected by the organ usually employed for that purpose. Many conditions of urine illustrate the former case; the latter is best exemplified by the occasional products sent off from the skin and lungs; but as the principle is general, all organs at times illustrate these interchanges of function. It might be enquired, by those who are inclined to doubt whether indeed diseases (so called) are generally placed in parts secondarily or yet more remotely affected, why Nature, so perfect in all her operations, should thus observe a plan which, to say the least of it, is calculated to withdraw attention from the true source of mischief, seeing how beautifully arranged natural phenomena in general are to convey useful instruction. The explanation is clear enough: nature is instructive only when the whole of the facts are examined. The study of those phenomena *merely,* which are most prominent, or which strike most on our external senses or our feelings, always leads to error.

The reason that various impressions which act on one organ, so frequently exhibit the manifestation in another, is plainly that (constituted as the body is) it is a beautiful conservative process. The excellence of this provision is easily educed by supposing that it were contrary. Suppose, for example, that every vesicular, pustular, or papular disease of the skin, selecting only such as *every one*

would admit to be dependent on derangement of the digestive organs, had been placed in the stomach, or any other part of the assimilating apparatus: why, we should have had every eruptive disease extremely dangerous, if not always fatal. Let any one try the same question with reference to the more common seats of *all* diseases, and the organs said to be primarily disordered; and he will find the enquiry resulting in this:—that those *impressions* which are most frequently the *originators* of disease, are *addressed* to one or other of the *most* important organs of the body; but that the disease (whether it be a tumour, or any other, usually subjected to surgeons, or one of those more commonly presented to the physician) is generally placed in a structure or organ less important than that on which the primarily disturbing influence had impinged. In order to test this principle fairly, the *relative* importance of organs must, of course, be the guide. No doubt instances do occur of diseases being seated in the organ on which the primarily disturbing influence impinged; but all these imply, excessive forces, or severe, dangerous, or fatal diseases; as accidents, violent mechanical influences, poisons, or matters powerfully impeding the *primary* functions of assimilation—as excessive indulgence, intemperance, &c. Take any extreme case. Let a lung be secondarily diseased; say from stomach or liver. Why nothing, it may be said, is worse than a diseased lung; but if the results of all cases of disease of the lung be fairly compared, how many of them, if I am not allowed to say recover, remain many years in a state not distinguishable from recovery; how many, who, although they remain

valetudinarians, live for years. But who ever lived at all, so to speak, with organic disease of the stomach? The reader, however, must think for himself; and, in the impossibility of developing the principles fully in a small volume, I only beg him to test the plan it unfolds, as a whole, fairly at the bedside of the patient.

A few other points of the "table of record" may be remarked on. The "chemical analyses" of the excretions is a field comparatively as yet unexplored; the mere analysis of any *one*, will, of course, only produce a measured degree of information. The *relative* conditions of them would probably help our progress exceedingly; but the time and chemical skill required, must render this department of enquiry very slow. Still there are many Establishments in which its neglect is altogether inexcusable; for it is proved to be extremely important in regard to the urine; and when we find even the physical or other obvious characters of excretions so often productive of useful information, it is probable that a knowledge of their *relative* constituents would be abundantly instructive. The other compartments of the table, with one exception, may be passed without remark; but that which suggests the "record of any alteration of function" during the progress of the case, is extremely important. Without this, it is impossible to obtain the crowning test of the accuracy of our reasoning, and the consequently beneficial application of it in future, either to the individual, or to others presenting similar conditions of the œconomy.

It is necessary also to the *medical* treatment, if you would apply it to the best advantage;—if a

patient is better, depend on it, that in a short time, if the medicine you were giving him was necessary, it will begin to be injurious. If you reflect on the matter, you will, I think, perceive that the very amendment has altered the relation of the medicine, at first prescribed, to the state of the œconomy. I do not mean that you should be changing a plan which is working well, every time you see your patient, *without reason;* but do not persist in giving medicine, when the organs are shewing a disposition to act of themselves. By watching the organs, you will see *why* your medicine does good; whereas, if you are content by merely recording that the patient has improved under this or that medicine, the great probability is that you will have learnt nothing from this case for future experience, but the *uncertainty* of routine practice; which no sooner induces you to rely on a certain remedy for certain *symptoms*, than, perhaps, in the very next case it disappoints you; a result which, in its daily multiplication, creates that humiliating uncertainty by which medical practice is too commonly characterized. In proceeding to offer a very few remarks on the management of different organs, I shall be obliged to say a little concerning medicine; I will, therefore, first give such general views in regard to its agency as have arisen out of some twenty-years' observation on two modes of treating disease—viz. that which rests the treatment (medicine inclusive) on the removal of the causes which disturb the œconomy, and constitute impediments to the operations of nature; and that which, although it may recognize the same principles, practically rests their application chiefly on the "Administration of Medicine."

CAP. IV.

OF THE ACTION OF MEDICINE.

Any one who will examine the manner in which medicine is generally spoken of in books, or in lectures, will perceive with how little definite notion of its action it is generally prescribed. Without even going thus far, he may learn much that will suggest the same idea, in observing the fashion! that exists in regard to medicine. One year we hear of creosote, another of prussic acid, a third of iodine, then preparations of bromine; then, of remedies which maintain a larger reputation, we hear of a considerable relinquishment of former replaced by novel applications of them, as in the various preparations of mercury, &c. Was there ever any thing comparable to this sort of fashion in any other branch of natural philosophy? and does it not confer a character of conjecture on the whole practice of medicine, in the highest degree repulsive to the thinking and judicious portion of mankind?

It is far from my intention to insinuate that we must, in every case, neglect the use of a remedy, whose power to achieve a certain object experience shall have sanctioned, until we can understand the

rationale of its action; but there surely is a wide difference between this and a wild empiricism, which would adopt, without any grounds, save the sanction of custom, this or that remedy, on scarcely any principle at all.

Science must make great advances, before we can undertand, *in every case*, the operation of medical substances. Many may for years, perhaps centuries, remain as obscure as most of them are at present; yet it does not appear difficult to obtain correct *general* ideas as to their relations to the œconomy; and this seems to be, not only the most auspicious method of cultivating a knowledge of their *particular* modes of action, but the means by which we shall soonest perceive the successful and judicious application of them.

The whole body may be said to be employed in assimilating certain substances received into it, and in rejecting certain others which are insusceptible of this assimilation; we cannot detach such relations from any form of matter submitted to the body. We have, therefore, only to consider the phenomena presented by food, and to trace them gradually through any connecting facts to those exhibited by medicine; and we shall, I think, evolve very useful general conclusions. Food is highly charged with nutritious or assimilable matter, which is generally converted into the various structures of the body; whilst the innutritious matter is as regularly excreted by various organs. In this rejection, we observe that the forms of matter rejected by the different organs are in a great degree characteristic, and suggestive, at all events, of special duties. The skin carrying off

chiefly certain products, the kidney *chiefly* others, and so on. We find, however, that different kinds of food are extremely different in the quantity that they contain of nutritious matter. Every one knows that beef, mutton, and bread, are more nutritious than turnips, carrots, or even potatoes.

Then there are certain matters which we call condiments, as pepper, mustard, vinegar, &c.; these contain a portion of assimilable material, no doubt, but still the quantity is again less; we could not live, for example, on either or all of them; and we observe that, in many persons, they very readily excite actions indicative, either of disturbance, or of efforts for their expulsion. Mustard, in certain doses, is emetic; vinegar very readily affects the skin or bowels; pepper, the urinary and, sometimes, the whole of the *assimilating* organs.

We have already, then, arrived at articles which, though food in the sense of condiments, are very easily converted into medicines, either by exalting their doses, or by administering them in particular states of the œconomy.

If, from such articles, we now proceed to consider the action of medicines generally, we shall observe, in regard to a vast number of them, little more than exaggerated exemplifications of the very facts we had observed in regard to food—but with the following remarkable difference: in food, we could demonstrate that part of it nourished the body, and that part of it was rejected. In the same manner, in respect to condiments, we had no doubt, though in much less degree than food, that they were *partly* nutritious, and partly otherwise; having some properties tending to excite actions ending only

in their expulsion from the œconomy. In medicine, however, we begin to lose the evidence of *nutritious* property, and observe that a very large number of medical substances seem to be followed by immediate activity in one or more organs, *apparently* to effect their expulsion; and we are the more disposed to view it in this way, from the fact—that if we exalt the dose, we render these actions more and more violent; until we reach a point, at which they tend to the destruction of the body.

That the object of these actions is the expulsion of something hurtful, we farther infer, because in every case where the substances alluded to have been taken, *and have not been followed by any such efforts*, the œconomy is variously disordered; and this in degrees represented, in different cases, by trivial indisposition, severe illness, up to the total suspension of the vital actions, as in the substances we call poison, or in articles not usually so called, but of which the doses have been exalted so as to be practically poisonous; in which the very substances excite actions so violent as to exhaust life; or, by their direct effect on the system, destroy the power of action altogether.

In the *apparent* expulsion of various medicines, we observe a proneness in different organs to be affected by different substances. Many are refused ingress, as it were, and immediately rejected by the stomach; others are admitted, but, perhaps, not into the circulation, and rejected by the bowels; others excite increase of bile, which may end in the same result. Others, again, are received into the circulation, but soon ejected by the kidney (diuretics), or skin (sudorifics, diaphoretics, &c.). In short, it is

impossible to resist the impression, that, in regard to an immense majority of medicines which are generally employed, or most usually relied on, they are presently followed by apparent endeavours at their expulsion. Now, I have used the term *apparent*, because, although in many cases we can by no means demonstrate that the identical principle contained in the medicine has been rejected by the organ supposed to have been excited for that purpose; yet, in regard to many things, it is demonstrable. Where matters are taken into the circuculation, the case is not so easy; though here the salts taken may be detected in the urine. Chemistry has not yet done all that it is *already* capable of, perhaps in the analysis of excretion; and it is probable that it must make considerable advances before it can recognize the ultimate analysis which vital chemistry has the power of effecting. It is not necessary, however, to assume, from the notorious facts to which I have adverted, that the matter expelled is bona fide that, *still less merely that*, which was represented by the medicine given; but that the effects occasioned have some relation to attempted expulsion of injurious matter, is rendered probable by the fact that we generally increase the expulsive action consequent on food, by taking any thing which the peculiar state of the system renders indigestible (that is, innutritious), or as certainly cause indisposition by its retention; being precisely what happens in regard to *a large class* of medical substances.

Notwithstanding so large a number of medical agents fall within the application of the foregoing remarks, I am well aware that there are many

substances which produce unequivocal effects on certain states of the system, whose action it may be difficult to reconcile with, or at least explain, on the foregoing views, and which may not be so reconcilable. The action of bark is a common but a very difficult example.

We do not perceive efforts made for its expulsion in *the manner* I have mentioned; whilst in some cases we *do* observe manifest improvement following its use. We can have little doubt that bark contains some assimilable matter; and it may in this manner correct certain disorders in the system, either by contact with the alimentary organs, or by being carried into the circulation, and again expelled by some excretion.

We know that a body may, from mere contact, have great power over chemical processes, whilst it remains itself unchanged—an exceedingly curious chemical phenomenon, of which gluten, in fermenting beer, is said to be an example. Certain it is that, in some cases, bark is very quickly expelled; whilst in others, in which it is said to have been inert, it has been removed from the bowels by spoonsful, apparently unaltered. The probability is that some principle is assimilated, and applied to the functions of the œconomy in correction of diseased action, in a manner at present not understood.

Whatever objection, however, bark and other substances may or may not form to the preceding views, in many cases their truth admits of demonstration; and on the whole they appear to me to be very probable, to say the very least of them. I am sure that they lead to good practical results; and especially in encouraging us to do what I have stated

as the desideratum, viz. to prescribe with more definite objects, as thus:

In observing the phenomena, we find them to be generally as stated; viz. that certain organs are excited by certain substances; but to this we occasionally meet with exceptions. I find that which I had prescribed as an aperient, not so acting, but affecting the kidney. Well, that is an exception, then; but I mark it, and as many as I can of the concurrent circumstances, because it may, in another case or at another time, be of the very first consequence to my patient that I should be enabled *designedly* to procure that which has here occurred contrary to my intention. That is, that if at any time I desire the kidney to act, and circumstances are present which render it inexpedient to employ diuretics, ordinarily so called, I may achieve my purpose by substituting an aperient, provided only I can simulate the circumstances of the former case so as to lull the vigilance of the bowels. What was here done in regard to the bowels and kidney, may in other cases be done in respect to any other two organs—a matter of great moment in many difficult cases, as I have frequently demonstrated*.

In the common mode of giving mercury, so as quickly "to affect the system," as it is called, the opium lulls the activity of the organs, and of the bowels especially. The skin, however,—on which almost every poison seems to act more or less—labours to throw off the mineral, and actually suc-

* This I noticed in the "Unity of the Body," some years since, deducing from theory, and proving from practice, that as the phenomena of Sympathy showed that disturbing influences travelled in every direction, so correcting influences might be transmitted in the same manner.

ceeds, as the watch worn by the patient sufficiently shews; but in so doing it must necessarily produce general activity at the surface, which must *as* necessarily relieve, quo ad, *for the moment*, *any* internal organ—the real key to the explanation of the use and abuse of this mineral; but were I to enlarge on the subject, I should only repeat what I have said of the action of mercury, and the practical explanation of it, in my Treatment of Inflammation*.

If the general action of medicine be that which the foregoing views would represent it to be, its beneficial effects would generally consist in exciting the actions of different organs of the body; and in so doing it would very conceivably induce the ejection not only of the injurious influence represented by the medicine, but that resulting from the *loaded condition of the organ*. But if we admit this in the fullest extent, still it is perfectly consistent with the continued operation of those causes which led to the condition of the organ so relieved; for this is entirely in harmony with the facts of observation. Medical influences frequently enable us to *relieve* disease; but there is no permanent health to be obtained but by the avoidance of those influences which, in different cases, are in daily operation; and which imply not the adoption of this or that medicine, but the avoidance of such things as are prejudicial. In cases, therefore, where, as in Tumours, there is evidence of sustained disorder, medicine will be in general most properly regarded as a stimulant, enabling different organs to relieve themselves, or the system at large, temporarily, by in-

* See "Medicine and Surgery One Inductive Science."

creased secretion; and thus become placed in a condition most favorable to the beneficial effects of a careful avoidance of *all influences* calculated to disturb them; whilst we are selecting and adjusting such necessary agencies as food, air, exercise, or any others as shall appear best calculated to adapt their several duties to their respective conditions, without *factitious stimulation*, so far as is consistent with a due conservation of the necessary powers of the œconomy. There is yet another point that should be noticed, which is, that medicine often produces the very evils against which it is intended to guard. Thus, no cases of torpid bowels are so obstinate or intractable as those which occur in persons who have been accustomed to rely only on medicine for the relief of this important function. Of these cases, I have met with many; and the gradual withdrawal of medicine is always an essential condition to the successful treatment of them.

No cases of diseased Liver are more insidious during life, more certainly destructive, or more demonstrable after death, than those which have followed on the abuse of mercury. I here speak considerately, and not on the evidence of cases wherein the mercury has been given for syphilitic, but for inflammatory affections. In regard to the liver, the recollection of this property of medicine is the more important, because not only patients, but many others, are so easily led into error.

Grease, fat, alcohol, mercury, are all stimulants to the liver; and when used carefully, they are very useful stimulants, their effect being the increase of the biliary secretion; but if they are continued, they excite the Liver, without

inducing its natural relief, secretion, and then become amongst the most certain causes of disease of this organ; although in many cases it occurs only after previous morbid actions in the skin, chest, and many other parts of the body.

I shall be obliged to recur shortly to this effect of medicine, in the few remarks which I propose to add in connection with the management of individual organs. In the mean time, I offer the following, as embodying the *general* views of the action of medical substances, of which I have endeavoured to give a faint sketch in the foregoing remarks.

1. Substances which are no sooner taken into the system than they appear to excite actions for their expulsion, and which either contain little or no assimilable matter, or, if they contain any, combined with principles which do not allow of its retention in the œconomy sufficiently long for the actions of assimilation; as jalap, aloes, elaterium, mercury, antimony, turpentine, &c. &c.

2. Substances, which, when received into the body, seem to contain certain portions of assimilable matter, and which, from their other properties, are retained sufficiently long for the assimilation of such matter; as bark, rhubarb in small doses, tonics, some bitters, &c.

3. Substances, which, when taken into the system, produce a direct effect on the nervous system, preventive of efforts at their expulsion; and which only come within the category of "substances exciting actions for their expulsion" when employed in small doses; as narcotics, all of which, in *small* doses, have stimulant properties, as opium, henbane, digitalis, or tea.

CAP. V.

HINTS ON THE MANAGEMENT OF DIFFERENT ORGANS.

In advocating a system of surgery or medicine, which rests its hope of making the best use of the knowledge we possess, or of extending its boundary, on a more careful investigation of the various organs, and on the correction of error or the restoration of balance in their respective functions,—I think it right to offer a few remarks on the treatment of some of the more important organs.

The reader will not imagine, I trust, that I am attempting any thing like a full discussion of so important a subject. My space allows me to do little more than offer a few hints; and these I shall restrict to points which I have been accustomed to regard as most essential, or which appear to be most neglected,

We observe, in the dietetic habits of individuals, as well as in the practice of medicine, two principles of action, of which the distinction, and the adjustment to the varying conditions of the œconomy, are of the first importance. I mean—firstly, that which endeavours to adapt the agenda to the power of an organ,—and, secondly, that which endeavours

to raise the power to the agenda. To illustrate the two in the most simple manner, I may say—that when a man, suffering from headache, loss of appetite, &c. the consequence of intemperance, begins his dinner by soda-water and brandy; then he attempts to raise the powers of the stomach to the task he wishes it to perform: but if, instead of this, he takes a cup of gruel and lies on his sofa, or keeps himself otherwise quiet; then he is adapting the thing "to be done" to the power of the organ.

When a man, subject to gout or indigestion, on returning from a convivial meeting, takes a pill at night—in apprehension of a morning attack of these maladies, he tries to raise the power to the agenda; when he has endeavoured to partake of such articles only as agree with him, he then adapts the agenda to the power.

From these, and a thousand other illustrations, presented daily in the practice of medicine, it must appear obvious that the adaptation of the agenda to the power should be the rule; and that, although many examples occur, in which the raising the power to the agenda by medical measures is not only safe, but, perhaps, preventive of future mischief —yet, no doubt can be entertained that they are to be regarded, in strict practice, as exceptions. In practice, the two principles are generally combined; but, it appears to me, with this serious error:—that much more pains are taken to raise the power than to adjust the agenda; whereas the right mode is to accommodate the agenda to the power, so far as that is practicable, and to regulate any endeavour to raise the power by *medical* measures, by such conditions as may be suggested by the various organs,

after we have carefully endeavoured at the adaptation of the agenda.

In carrying out the application of the foregoing principles, the first thing is to relieve the animal œconomy from all injurious influences; whether those which seem to disturb by forces acting on the nervous system generally, or by an agency directed to particular organs.

In thus guarding the system from disturbance, we must recollect that injurious influences of a *relative* kind, such as exacting the digestion of food, of such quality or quantity as would in health be natural, may, in a state of disorder, be just as truly injurious as any influences of an *absolute* kind; such as bad air, intemperate habits, alcohol, &c. would be in more healthy conditions of the œconomy.

The management of

The Stomach

is the first thing we desire to accomplish; not only on account of its abstract importance, but as forming the channel through which we are obliged, in so many cases, to direct our means to relieve other organs.

Defective assimilation may refer specially to any one of its numerous processes; to the duodenum, liver, or any other part. The selection of the diet may be regulated by a desire to adapt it to the stomach, or to the *special* condition of some other organ; still it will, of course, modify the matter subjected to the stomach; so that, in some cases, we are obliged to be extremely particular in regard to

diet, where the stomach has little or nothing the matter with it.

This distinction is important, and its explanation to patients necessary, both to render them intelligent coadjutors in the treatment, and to disabuse them from the impression they are too apt to entertain—that a man, who is particular in the diet of his patients, refers all diseases to the stomach.

I may now, then, offer a remark or two on the stomach more particularly. In all cases, the first point is to *try as simple* a diet as you can; as plain meat, bread, farinaceous vegetable, and so on.

By having few articles in a single day, you obtain two advantages: one, that if they are such as I have stated, they will, in general, be the most likely not to offend; the other, that if any of them do not agree, you will have less difficulty in detecting the article,—which difficulty will, of course, be in a direct ratio with the variety.

In all cases of difficulty, the spreading the food over a large surface, by exceedingly careful comminution, and by reduction of quantity *at a time*, is of great advantage.

In nature, whatever animal we examine, we find that, as the food is wholly vegetable, it is subjected to a much larger surface than when it is wholly animal; and that such as feed on both kinds of food, have an intermediate extent of surface.

This suggests very strongly, that as the material departs more from the nature of an animal, so it requires a greater extent of surface, or more time for its conversion; and it would seem as if in many

cases the necessity for that greater elaboration, which here depends on the nature of the matter to be digested, is, in disorder, represented, in regard to food generally, by deficient power. Whether this be the explanation or not, it is certain that in many severe cases you may, by care as to comminution and distribution through the day, accomplish the digestion of a very acceptable quantity of food, where any thing approaching to ordinary quantity, *at one time*, would be out of the question. Ordinarily, the application of this principle is sufficiently met by comparatively frequent and moderate meals; but, in some cases, not only is an extremely stringent application of it beneficial, but that on which alone success appears to depend.

The most remarkable, successful application of this principle that I ever saw, was in a gentleman who consulted me some years since, and who resided a long distance in the country. The whole case was very instructive; but I will only here state its main features. Intense pain after eating, frequent rejection of food; for which symptoms all sorts of medicines, the usual round of tonics, concluding with brandy and black drop, had been taken to a pernicious extent. He consulted a very eminent surgeon here, who first prescribed a tonic; but, on the patient complainingly stating that he did not desire a tonic—having taken plenty of that class of medicine—but that he wished his opinion on the *nature* of his case, the surgeon then examined him carefully; but concluded by saying that he did not know what was the matter. On his consulting me, I found, on examination of the abdomen, that he was so exquisitely tender over the whole epi-

gastric and right hypochondriac region, that he would not allow me to touch him; therefore, although he complained of excessive pain, I could not make out whether its seat were the pylorus or not. I, therefore, heard the history of the case, gave a very guarded prognosis; but still, from the history, was led to express a hope that there might be no disease of the pylorus. I then advised him to pursue a particular plan of diet, which chiefly consisted of endeavours to give his stomach *exceedingly little to do at a time*, and to spread it over as large a surface as we could by previous careful comminution. I then advised him to check these suggestions by consultation with a distinguished physician, Dr. Prout, who expressed his approbation of the plan, but who added his apprehension as to the result, from the too probable disease of the pylorus.

The gentleman, however, recovered; his *medical* treatment consisted *chiefly* of a gradual relinquishment of black drop and other narcotics, to which his sufferings had obliged him to have recourse.

I mention the foregoing because it was regarded by every body, from its duration (it had been coming on for about two years) and obstinacy, as particularly desperate.

There are several similar cases scattered about medical works; but the treatment appears in all to have been reducible to the same principle.

In some, the food has been fluid. This, of course, carries out the principle of extreme division; but there are many occasions in which fluid food is inadmissible. Much deranged stomachs can seldom digest milk; and there is something in the culinary

chemistry of soup and broth, which frequently render them offensive or insufficient. Still they do very well in certain cases. I am here, however, alluding to cases of very much greater difficulty, than will be ordinarily encountered in cases of tumour; where, in general, the adjustment of the diet is not really difficult, if the patient and the medical attendant will only take the necessary trouble.

Every one who has been led to investigate the subject of diet for many years, will necessarily have obtained some useful generalization with regard to different kinds of food in different cases; but in my own practice, I never apply these farther than in laying down the plan that I think best at the commencement of the case; I do not allow it in the least degree to diminish that vigilance which I think necessary to establish, *in each case*, a diet absolutely adapted to it. Having commenced by a plan which appears, in a general sense, applicable to the case, I check its propriety by rigid observation, in which I instruct the patient to co-operate.

In many instances, I direct that articles taken should be set down in a little book, for that purpose, with the dates; and on my next visit, when I have examined the condition of the patient, I underline such articles as I desire to forbid, and place a mark opposite such articles as I consider doubtful, and desire the patient to observe his feelings or any other sequences, should he take them. I begin with as few privations as I think consistent with the plain requisitions of the case.

It is always desirable to avoid privations that are really unnecessary; and if the patient doubts the necessity of any particular prohibition, I am

always ready to allow him to *test* its necessity. I am sure that, by this entire dismissal of all reserve, you often do more to convince your patient that you are in earnest, than by any other mode of proceeding; by allowing him any test he chooses of your correctness, you not only obviously show him that you are sincere, but do much towards obtaining that confidence which is so necessary if you are to work together.

By the simple plan which I have mentioned, it is astonishing how soon, in many cases, you hit off a diet which you had not been able previously to adjust. I could mention a great many examples of it, and of its instant effect on morbid deposition. I saw a man, a few days since, who had, some years ago, an extraordinary disease of the tongue; and it was curious enough that I had three cases under my care at the same time, all of which seemed to be gradual shadings, as it were, from simple deposition with ulceration, exalted in the third case into genuine cancer. The case I am at present alluding to presented itself as one in which there seemed to be a considerable tumour, occupying the greater part of the surface of the tongue. It felt remarkably firm; but, on careful examination, not exactly hard; and, on pressing it deeply, I thought that I distinguished fluctuation through a very dense covering. I therefore made a small incision, and let out a quantity of rather unhealthy matter: still there remained a good deal of thickening; and this gradually increased, and at length began to assume rather a tubercular appearance. The patient was a young man; and I cannot say that it had put on the characters of a decidedly malignant disease; but

it was very hard. I began to be apprehensive, and felt anxious to get rid of the disease. I accordingly took every pains I could with his diet, impressed on him the danger of trifling with a malady of that kind; and I have reason to believe that he was very steady. He was directed to speak as little as possible, which was the only *local* application. The disease now began to mend; a considerable portion of the thickening became absorbed; but the absorption having proceeded to a certain point, the deposition became stationary, and for a time foiled all endeavours made to effect further change. I then made him keep a written account of every thing he took, directing such alterations, as the articles, coupled with my conviction that *something* was wrong, suggested. In about ten or twelve days, the absorption re-commenced, and went on progressively to complete recovery. I could relate a vast number of cases illustrating the same point.

A very little attention to the subject will convince any man how little is done in the regulation of diet, compared to what is not only practicable but easy. There are many things taken every day that, in different cases, constitute all the difficulty. No two articles, equally general, are such common disturbers as grease and sugar; and yet I have known many cases of exceedingly deranged stomach, where no caution whatever had been given on the subject. But I shall speak more particularly of such matters in connection with the liver.

I avoid attempting to designate those cases in which a diet chiefly animal, or chiefly vegetable or farinaceous, may be proper. A condition of stomach *apparently* the same will in one case require a fari-

naceous, in another a more animal diet. I know not any sign by which either peculiarity can be certainly predicted; but where there is any doubt, I always begin with the farinaceous. In some cases, it has *at first* many advantages: irritable stomachs frequently bear with it longer and give more time; farinaceous food, if undigested (a condition always to be borne in mind at the commencement), will occasion less disturbance than undigested *animal* food. But even where animal food appears unsuitable, *if you forbid every thing that is unnecessary* (a golden rule), take care that the animal food is *tender*, and mix it with a fair quantity of farinaceous matter, you will often succeed in obviating its objections, and find it attended with great advantage. No general rule will obviate the necessity of extremely vigilant observation in each case; and a very little drilling, in really close observation, will soon enable any man to generalize for himself quite as much as is desirable. In many patients, it is difficult to separate the idea of a *precise* from that of a *spare* diet; so that they think they are carrying out your views, in proportion as they restrict themselves in quantity. This is an important mistake.

Whilst you are adjusting the diet, unquestionably it is a security to keep on the sparing side as to quantity; but when you have gained this point, there is nothing more desirable, in cases where the regulation of the *stomach* is your object, than that the patient should take as much as can be perfectly digested; less than this, is, *so far*, throwing away the element of power; more, is obviously distributing elements of mischief through every organ of the body. The reader will, I trust, not misconstrue this paragraph

as overlooking the excellence of habitual moderation, or as contradicting such directions as to quantity which may be necessary for relief of *particular* organs.

It must be obvious that, in the proportioning of the quantity to the appetite and power of digestion, it is absurd to regard that appetite as the suggestion of nature which is the result of *indefinite* quantities of stimulant condiments, as pepper, especially cayenne, mustard, &c. These, therefore, should be withheld, or allowed only in extreme moderation. However ill adapted the present habits of the patient may be to ensure success, it is, nevertheless, very often necessary to proceed *gradually* in changing them. With regard to alcohol in its various forms, this is well understood; but it is no less true in regard to sugar, butter, and many other things.

On the other hand, it is equally certain, that, if you proceed with the foregoing caution, no bad habit, however inveterate, is irremediable. Of this I am convinced, from the success of many cases, including very severe forms of laudanum and dram-drinking.

In avoiding injurious influences operating on the stomach, we include many which are *first* directed to other organs, and which will be spoken of in connexion with them; the skin especially: but I mention tobacco in this place, because, although its effects are general, and often complicated with other agencies, yet I think it *more commonly* acts primarily on the stomach.

Tobacco, every one knows, is a virulent poison; and the wonder is, not that it should do mischief,

but that the mucous membranes, notwithstanding their ready power of relieving themselves by secretion, should be so much exposed to it with such apparent impunity. That in many persons it is *really* prejudicial, I, from ample experience, could easily prove by the relation of cases; and although I have generally seen it act through the stomach, yet there is no organ in the body of whose functions we have any knowledge, that I have not seen seriously disturbed by the use of tobacco.

Of the two forms in which it is generally used, I know not which may be the more prejudicial; nor would I assert that there is no plan of sustained moderation which may confer impunity.

I have somewhere seen a Report on the effects of tobacco on those engaged in its *manufacture* in France, from which it would appear that so far it is not unhealthy. In this country I should say that those engaged in that occupation, so far as I have observed, have an unhealthy appearance.

Negative instances, however, are of no weight against any well-sustained affirmative of the contrary; and of this there are plenty of examples. Smoking, or taking snuff, are each capable, in different cases, of disturbing any organ; snuff, especially, when taken with the stomach empty, seems to me to act chiefly on the digestive organs; smoking more commonly on those of respiration and circulation.

Whenever, therefore, I have a case requiring unusual circumspection, as to disturbing causes, I press the gradual reduction of tobacco, with a view to its relinquishment—if necessary.

All writers on diet very properly impress the im-

portance of small quantities of doubtful articles; and this is so true, even with regard to some of the most wholesome kinds of food in particular cases, that we are sometimes obliged to give doses of food almost as if we were giving medicine. In the absence of my limits enabling me to put forth the results of my experience on this subject, I can confidently recommend to the practitioner, the adoption of the plan of keeping the account I have mentioned; not only as rendering further instruction unnecessary, but because the fullest possible directions would not enable me to obviate the necessity of that availing vigilance which it so successfully applies.

I know of no article scarcely, in the whole diet scale of civilized societies, that I have not known to disagree with particular individuals; and these peculiarities are, in certain persons, entirely inexplicable. Dr. Prout mentions a patient, in whom mutton appeared to be little less than poisonous; this seemed to have been a permanent peculiarity; but peculiarities dependant on temporary disorder are occasionally not less strange. In a remarkable example, which fell within my own knowledge, it appeared that apparently good bread was an efficient cause of disturbance: a most obstinate disorder of the alimentary canal had continued for many weeks; the patient came to town and consulted many eminent men, without obtaining other than temporary relief; when his medical attendant in the country detected, as he thought, that bread disagreed with his patient; he relinquished its use for a short time, and straightway recovered.

In this too, the mere change of residence must have led to a change of bread more than once, and

perhaps several times, during the progress of the case. It would be impossible to pursue this subject through its multiplied exemplifications. Almost any treatise on diet furnishes examples of occasional peculiarity; much may be learned from the patient, and the Record will soon supply the rest, if necessary.

In my own practice, I am extremely cautious about medicine, whilst I am adjusting the diet; yet a thoroughly good principle may be abused. If any particular organ is loaded, it must be relieved. If you can make a patient comfortable, by giving him a medicine which relieves a particular symptom, and which seems to help the digestion, it is, perhaps, right to do so *until you have adjusted his diet.* It must be remembered, however, that he will rarely get well by any *medicine* directed specially to the stomach. In some cases, the neutralization of acid is useful; it is better than risking its further progress, perhaps; still the use of alkalies has multiform ill consequences, unless they are administered with due care. They frequently *increase* the *formation* of acid, and this necessitates an increase of alkali; one of your views, in its neutralization, may be to protect the kidney from disturbance; but, if you are not cautious, the alkali will create a fresh source of annoyance and danger. There is not the smallest doubt that many cases of serious disorder of the kidney have arisen from the *abuse* of alkalies directed to the relief of acidity in the stomach.

If the stomach generates acid, I do all I can to induce such an increase of exercise as the case admits of. This is a real tonic; but, like others, requires its dose, if I may so call it, to be appor-

tioned to the case. There is, indeed, seldom fear of a patient taking too much of it.

When, however, it is fairly adjusted to the general powers, as well as to any special condition of the patient, it is so essential an element in bringing back the various organs to their respective duties, that it is generally quite indispensable.

I am in the habit of assuring patients that exercise, although not so necessary to mere existence, is quite as necessary to health as food. And, now I am on the subject, I may observe, that I *in general* add "and in all weathers." I know some people will start at this, and think of many cases, and of periods of the year, when the application of such a rule would present little but exceptions. That may be; but still I have learnt that the rule is susceptible of much more extensive application than is usually admitted; and, further, that unless it be fairly pressed to the full extent of such application, it too often ends, so far as any efficient degree of exercise is concerned, in its neglect altogether.

Inaptitude for exertion, measured power, or great deficiency of it, must be met by correspondingly small quantities at the commencement. Climate, if cold, by *really warm clothing;* by thick *warm* lamb's-wool stockings and plenty of flannel—a very obvious requisition, but one which, amongst ladies, is often attended with considerable difficulty. The walking with frequent stoppages, as in shopping or making calls, is so far from carrying out the true principle of exercise, that, in cold weather, it often does as much harm as good. The surface is constantly exposed to cold, and patients get the exertion without the equalization of the circulation. Exercise should

be taken, so that the circulation should be *gradually* rendered active at the surface; and its dose may be often judged of, and not inaccurately, in weakly people (as Armstrong expresses it, in his beautiful poem), "By the first moisture of the brow."

In its tendency to equalize the circulation, exercise is directly opposed to local congestion; and, in thus invigorating the organs generally, gives power to the stomach also. If there be acid to be got rid of, it is the most certain mode of giving increased activity to the organ whose natural duty it is to separate that principle from the body; I mean "the skin."

Besides the special inconveniences attached to the too ready use of alkalies, there are others not less serious to the patient, though of a more general kind. The object is, of course, not *merely* to neutralize the acid, but to prevent its formation; to this, all the alkali in the world, in a direct sense and *so* far as the stomach is concerned, is worse than useless; this object can be only accomplished by adapting the diet to the power and condition of the stomach; and, whatever of difficulty there is in effecting this, it is indefinitely increased where patients are armed with a remedy which gives them temporary relief from the results of any indiscretion.

Few points require more caution than the use of medicine whilst you are adjusting diet; and I am persuaded that the homœopathists do much good, very often, by combining a careful diet with that practical avoidance of medicine, to which their infinitesimal doses are a close and wholesome approximation.

The judicious will avoid extremes, and not hesi-

tate to prescribe medicines where there is a definite object. I would only beg the young practitioner not to fog his case, by prescribing, as a matter of course, or for no better reason than because he has been recommended to do so by fashionable authority.

The use and abuse of alcohol, in all its forms, is so generally understood, that I have only to observe that it should be regarded as medicine, and prescribed or not, according to the condition of the stomach and the *former habits* of the individual. In some cases, alcohol proves beneficial in moderate doses in one form, when it fails, or proves injurious, in another. I have occasionally seen stout claret do good, where other wines either excited or disagreed; alcohol, however, and some other matters, as fatty and saccharine material, have appeared to evince, in my experience, much more important relations to some other organs; and although, of course, they are very important in regard to the stomach also, still I think it better to connect them with organs, to the relief of whose functions they appear more specially applicable.

The following, then, are the chief points I desire to impress in the management of the stomach.

1. Moderation.

2. The exclusion of unnecessary variety at a single meal, or (during investigation) in a single day.

3. A careful adjustment of animal or farinaceous food *to the case.*

4. Extension of surface of the food by minute comminution or otherwise extreme division.

5. Avoidance of *all* interfering influences, whe-

ther excessive indulgence in condiments, or questionable habits, as snuffing, smoking, &c.

6. Determined exercise on foot, horseback, or by gestation.

7. The avoidance of hasty generalization; and, until your plan is tolerably defined, desiring your patient to keep a book of diet; in order that you may test your selection, or that of your patient, in detail.

The Liver

is, in every point of view, a very important organ in the treatment of every disease; and in a very great number of cases of tumours. The peculiar relations of the liver; the large quantity of blood with which it has to deal; the flood that it is constantly pouring into the chest, from which *it ought* to have extracted its characteristic secretion (the bile); the important offices which the bile executes, for though we know little more, we are certain that they are *important;* bring this enormous organ, not only into direct, but into very important, relation with the whole of the viscera of the body. In this country, the climate and the habits of the people render the knowledge of the character and peculiarities of this organ most essential in all diseases; whilst, in a large number, the detection of its actual condition is the only road to success. No organ is nearly *so silent or insidious* in its conduct of disease.

Over and over again, I have known it thoroughly altered in structure, and yet without there having been any *symptom* to excite suspicion. In a case, where its disease had arisen from the abuse of mer-

cury, and in which I had predicted it from the knowledge of that circumstance, no other fact in the least degree suggested the real condition of the organ; nay, more, on the evacuations at one period suggesting deficient biliary secretion, they resumed their ordinary appearance, on the administration of a mild dose or two of aloetic aperients without mercury, just as they will sometimes do in healthy conditions of the organ. I have known the liver at fault, when the symptoms have always, for years, been referred exclusively to the stomach. The frequent disturbance of the heart and lungs, from congested and other disorded conditions of the liver, are now universally admitted; although it is not many years ago, that a Reviewer thought it proper to inveigh against such an opinion as a dangerous doctrine.

All these things I have endeavoured to impress for some years; but I still find that men will content themselves in founding their decision of the state of this organ on symptoms. Now, it is true enough that a disordered or diseased liver will very often, perhaps commonly, afford signs indicative of such conditions; but the absence of such signs is no proof whatever of the absence of such disorder or disease. I know of no mode of educing the true state of the liver, but by a careful history of the patient, in order to ascertain to what injurious influences it has at any time been exposed; and whether any influences, of a more *ordinary* kind, have been so *exalted* as to become injurious. Now, some of both kinds are well known, or, at all events, easily demonstrable; amongst these, I may mention alcohol, sedentary habits, and that free use of greasy,

fatty, and saccharine matter, which characterizes the habits of most people.

The prejudicial influence of alcohol on the animal œconomy, and its special tendency to produce disease of the liver, is so generally known, that I need not occupy the reader's time by a sketch even of any of the numberless victims which I have known sacrificed by the too free use of it. With regard to saccharine and fatty matter, I feel very anxious to impress my conviction of the mischief they occasionally produce, and the extraordinary results which follow moderate use, or relinquishment of them, as the case may require.

In surveying the animal œconomy and the elementary principles of which it is composed, as carbon, hydrogen, oxygen, and nitrogen, it is impossible not to be impressed with the immense quantity of charcoal taken into the body, and the apparently elaborate provisions for regulating the quantity retained there, represented by the various excreting organs.

We not only observe that extremely important and bulky organs are almost wholly employed in carrying out charcoal from the body, but organs whose functions are otherwise specially directed, all eject also an abundance of this material.

The lungs alone expel from the body a great many ounces weight of charcoal in twenty-four hours. The liver is almost wholly engaged in the same office; the principle of bile (cholesterine) being, in round terms, about eighty per cent. charcoal. Those organs, that is the liver and lungs, may, therefore, perhaps, be regarded as the special decarbonizing agents; but the skin also carries out

large quantities of charcoal, in acetic and lactic acid, its oleaginous secretion; and again in the form of carbonic acid. Then again the kidney, an organ which ejects a large quantity of nitrogen from the system, still lends its aid also in carrying out a considerable quantity of charcoal. The adjustment of this principle then (apart from all theory) must evidently be a matter of great importance; for it is plain that our food is rich in charcoal, and that, therefore, we must take in an enormous quantity; and that, every organ being more or less engaged in carrying out charcoal, *we ought* to get rid of an enormous quantity. Now, I am particularly anxious to impress the reader with this fact, unincumbered with any theory, either of Dr. Crauford's or Liebig, or any of the numerous intermediate writers. What I have just stated are facts susceptible of demonstration. We can analyze the food, and we can analyze the breath, the bile, the secretions of the skin, kidney, &c. and bring out the charcoal; and, therefore, that the regulation of the quantity of that material in the system is important, is an unavoidable result; or else we get into an absurdity, viz. that all these provisions of which I have spoken are useless. Now, a mathematician can give no better proof than this.

In the regulation, therefore, of disordered liver, I try all I can to diminish the quantity of charcoal, except such as is contained in *necessary* food. A man may eat meat without fat; butter is unnecessary; so is sugar; and in this way, without totally depriving the system of any thing really necessary to health, you may relieve the liver in a very material degree. The benefit arising from the prohibition

of these alone, where it is stringent in proportion to the exigencies of the case, will soon excite surprise in any one who accompanies them with an otherwise rational diet; and in those cases in which a vigilant attention to the general health—such as it is my object in this book to sketch—has been followed by the absorption of tumours, which I suppose I must not say were malignant, but which I verily believe to have been so, no one point of diet has appeared to me so essential as the exclusion of fatty and saccharine matter. If a patient has a tumour, and that tumour becomes stationary, and then begins to diminish under a certain plan, and then it again enlarges under the same plan—with the exception that your patient confesses to having indulged in certain articles that you have forbidden, and this happens more than once in the same case; what further proof can you have, that the plan was influential in diminishing the growth of the tumour, and that its renewed growth was the result of the aberration from such plan.

Now people will cavil, and some will say, "How do you know that your patient does not deceive you," &c. I reply—that people seldom deceive, when they can have no interest in deceiving; and, if a patient determines to be candid in confessing to disobedience of *any* instructions, I do not see why he should name any other than the true. The avoidance of alcohol, fatty and saccharine matter, I hold to be the great mode of relieving the function of the liver; whilst pure air, and the persistent adoption of such exercise daily, either on foot or horseback, as the particular case admits of, are the chief elements of increasing power.

Having, for years, observed the great importance of relieving the system in cases where the liver appeared congested, or otherwise at fault, from redundant carbon, by cutting off condiments rich in that material; and having seen in cases of tumour, both of benign and malignant character, well-marked derangement of the liver; wherein the tumour had been relieved or dispersed, as the case might be, under a diet vigilantly excluding unnecessary quantities of carbon; I was exceedingly pleased to see, in Dr. Prout's admirable work on the kidney and on digestion, that, in concluding his remarks on fatty substances, he observes—that it would not surprise him if, by and by, it were discovered that some of the most malignant diseases were found to be connected with the defective assimilation of oleaginous and fatty matters.

This is to me very interesting; first, from the pleasure I feel in having the constructively concurrent opinion of this excellent physician; but especially on the increasing probability, that it suggests that there is something very important in connection with the faulty assimilation of these substances. Dr. Prout is considering, in the work in question, the influence of indigestion on the kidney, and its so called diseases; but which diseases he refers (qua causation) to the digestive organs, and consistently plants there, for the most part, his remedies. This leads him to regard the defective assimilation of fatty matter as an important element in disease; and he concludes by the remark to which I have referred.

On the other hand, impelled by various cases

and dissections, to investigate injurious influences acting on the liver, in consequence of finding it so often diseased when not suspected; and believing that tranquillity of an organ is best insured by moderate labour, I endeavour to abridge the labour by diminishing the quantity of that principle, the separation of which I believe, nay, I know, to be *one* of the functions of the liver; and strike at sugar and fatty substances, not because their faulty assimilation deranges the kidney, but because they have one property in commmon, that of being rich in carbon. The result is that the most signal changes which I have hitherto produced in any tumours of *supposed* malignancy have occurred in cases wherein the treatment included the prohibition of these principles; a practical coincidence with Dr. Prout; of great interest, from the very circumstance that the impression seems to have reached us by entirely different channels.

This is one of the thousand occurrences which suggest that medicine and surgery must be one inductive science, if they are to be any science at all; but I will not enlarge on the subject, because we are only speaking of one organ; and although in different cases the regulation of one may be the special object, yet it is by the regulation and correction of the whole, that we can alone hope to obtain any real command over disease.

I had drawn out some few cases in detail, illustrative of the *insidious* nature of disorders of the liver, so far as symptoms are concerned, and yet showing how correctly they might be inferred when you can obtain the history of all injurious influences to which the liver may have been ex-

posed. To compress the volume, I have been obliged to restrict myself to brief notices of two or three of them.

A lady, whose symptoms were all referred to her chest—who had been many months treated accordingly, came up to London to consult me. She stated that her chief distress arose from difficulty of breathing, sometimes so extreme as to occasion a sense of suffocation. The difficulty of respiration was accompanied by a sensation of weight and oppression about the chest. Her symptoms varied, and sometimes she breathed freely and naturally.

She had been subject to slight bleeding from the nose, and she had once or twice coughed up a little blood, which she was occasionally doing at this time, but not in any quantity. She was strongly impressed with the idea that her right lung was much diseased, because she had been so informed; and she had been recommended to reside in a warm climate. I examined her chest very carefully; I could find no disease of the lungs, nor any thing worth mentioning, except that the sound on *percussion*, at the upper part of the right side of the chest was rather dull. The heart was noisy and rather irritable. She had resided for a short time, at a former period of her life, in one of the West India islands, where she had fever, reported to have been a bilious remittent. Her habits had been sedentary; she appeared very excitable, exceedingly averse to take any medicine, and interested chiefly as to the climate which I should recommend for her residence. Her bowels, when left to themselves, were inclined to be torpid, but acted readily on very gentle excitation, with

tendency to diarrhœa. Her complexion fair; though, when I *first* saw her, very slightly tinged with yellow. My opinion was that the disturbance of her respiratory organs was entirely dependent on her liver; that even admitting that the upper part of the right lung was consolidated—a point on which she seemed determined,—it would not influence my plan of treatment; that the whole case turned on setting her liver to rights, and in adopting habits calculated to protect it from future annoyance; and, by way of beginning, I suggested a lodging out of the smoke of London. Now, the whole history of this case was very interesting; and to me not the less, from my having predicted exactly what we found after death; for as the lady refused to take the medicine prescribed, or indeed to pursue any other measure recommended, she gradually got worse and worse, until it was very evident that her death could not be far distant.

She now became alarmed, and just as anxious to do what might be recommended, as she had been before determined to do nothing. Notwithstanding that she had evidently let the opportunity pass of doing good, I tried to act on the liver, and succeeded in getting her stomach, which had become extremely irritable, to retain two mild doses of calomel with an aromatic; the result of which was the evacuation of an enormous quantity of bile, with decided and complete relief to her pulmonary symptoms. The irritability of the stomach, however, still continued, and she gradually sunk, after having ejected, towards the last, both by the bowels and stomach, an enormous quantity of bile. Examination discovered *the lungs perfectly sound;* the dull

sound at the upper part of the right side of the chest had been occasioned by some adhesion between the pleura, the result of some former inflammatory attack; the heart was slightly hypertrophied, and there was some adhesion of the pericardium. The liver was thoroughly diseased (what is called the nutmeg liver); and, notwithstanding that she had vomited so much bile, the gall bladder was quite full of that fluid. The entire relief to her chest symptoms, even though the heart had become organically diseased, shews what may be done by relieving the liver in such a case; but I have seen this in other cases also.

Many years ago, a gentleman, apparently in good health (whom several surgeons had carefully examined, and especially with regard to the state of his abdominal viscera, and who had no idea but that they were perfectly sound), had a serious operation performed on him by myself, from which he so far recovered, that the wound had healed to a mere point, when a circumstance occurred in his family which exceedingly distressed him. The next day, a spot of erysipelas appeared in the site of the wound, which, extending, was accompanied by severe general disturbance, of which he died in about a week from the attack of erysipelas. Every organ was found sound, except the liver, which was thoroughly diseased throughout. This gentleman had in early life lived *rather* freely, but for *twenty years* before his death had led a temperate life, drinking beer generally, wine occasionally, and both in moderation; but his habits were sedentary, and he was rather corpulent.

A medical gentleman died, last year, of the

bursting of an aneurism of the aorta. He had at various times of his life been indisposed, his *symptoms* being generally, and on most occasions very emphatically, those of irritable and otherwise disordered *stomach*. He knew my opinions well. We had been intimate for a great many years; and from time to time he had had opportunities of observing many of the cases and dissections from which I had originally deduced them. The bearing on his own case, with reference to the power that I alleged disturbance of the liver to exert, not only in producing *disorder* of stomach and chest, but organic *disease* of the latter, had at various times, in our social as well as our professional intercourse, been the subject of conversation.

I used to tell him, when his stomach was so clamorous, that I was sure that his liver was chiefly wrong; but that I could not be certain that it was the primary organ, but that still I thought so. My reason was founded on his mode of life; for, though temperate, he lived generously, especially in the earlier part of our friendship. He was a stout, largely-framed man, and his exercise so measured that I had for years been advising him to be more particular in that respect.

He had been seen at different times of his life by several medical men, and the stomach was the organ more generally blamed: there was one thing, however, which induced me to hold to the opinion I always held in regard to him (notwithstanding former differences with some and cold assent from others), viz. that whatever form his indisposition assumed, he was always relieved by a copious biliary secretion, and seldom until that was ob-

tained. Shortly before his death he had a very curious attack, followed by considerable disturbance of the circulation; the pulse became very full, and for the first time suggested fears of hypertrophy of the heart. He was kept quiet; his liver was acted on at first with a certain amendment only; but, in a few days, with almost entire relief to the circulation, his pulse having resumed its ordinary character. He had had no symptoms about the liver except that, on carefully examining its site, he was a little tender when I pressed (as I thought) on his gall bladder; but even this tenderness had subsided, as I ascertained at eight o'clock in the evening, previously to the bursting of the aneurism, of which he died in an instant, at two the following morning.

Examination discovered an aneurism of the aorta, which had burst into the chest, out of which were taken four pounds of coagulum. The blood appeared to have been recently pumped between the coats of the artery, so that the outer was separated from the inner and middle coat, from just without the pericardium to so considerable a distance as to involve nearly the whole thoracic portion of the vessel. What the state of the biliary apparatus had been for a long period previous to death, may be inferred from the fact that the gall bladder was actually quite full of gall-stones, suggesting to the examiners that the bile must have passed directly from the liver to the intestines. Now, this gentleman had had symptoms at times which certainly do affect those who have gall-stones; but he never in his life had one that was distinctive of that complaint; and yet I never saw a gall bladder so crammed, if I may be allowed the expression. I

could relate a very large number of cases, illustrating this important point; I mean the absolute futility of attempting to educe the real state of the liver from *symptoms*, or from any thing but a careful enquiry into the influences to which it has been subjected, and such a previous knowledge of the subject, by reading or experience, as shall enable the medical examinator temperately to adjust his conclusion to the evidence whence it is deduced. In my remarks on prescribing medicine to act on the liver, I must necessarily be brief.

If in a really torpid or congested condition of the organ, medicines are prescribed which readily affect other organs, or which affect the bowels in common, the liver will very often not act, or rather act just enough to deceive. I have seen many cases in which a patient's liver has been reputed to have been right, because several doses of calomel had been given with considerable action of the bowels; when a single dose, so combined with a narcotic as to allow of a *slowly progressing* action of the bowels without purging, has brought away an enormous quantity of bile, with instant and decided relief. This sort of thing happened in a desperate, but successful case, of what would be called typhoid pneumonia, where the bowels had acted only the night previously, as the gentleman in attendance assured me, nearly twenty times without the slightest relief. In fact, the bowels here labour to compensate for an inactive liver; and this is one form (and in this country a very common one) of diarrhœa.

It is very useful to remember that almost any aperient medicine will excite the liver, if we retard its progress through the alimentary canal. This

we accomplish very sufficiently when we combine aloes, colocynth, scammony, gamboge, or other aperient, with small doses of opium or other narcotics. It requires, however, different proportions in different individuals; the general plan being to combine as much of the aperient as would ordinarily act in the usual manner, with about three or four grains of confect. opii, or with an equivalent dose of any other form of narcotic; adjusting the narcotic, as the bowels are too active or otherwise. The same object is often very readily obtained by repeating *minute doses* of aperient medicine without any narcotic; and this is useful in many cases when narcotics disagree. Almost any one who will take a full dose of a medicine which he knows to agree with him, but which acts too quickly, may generally convert it into a bilious aperient by dividing it into three or more portions, and taking it (with gruel or such like diet) every three or four hours, until it operates. The object seems simply, as I have observed, to secure the *slow* progress of the medicine through the alimentary canal.

These plans are very useful in many cases where mercury disturbs, or is otherwise objectionable; such as in persons in whom the disorder of the liver has been traced to an abuse of mercury (a numerous class); persons in whom mercury, in any dose, acts almost like a poison (by no means uncommon); individuals, again, in whom, without seriously disturbing, all mercurial preparations seem to be particularly depressing; and many other conditions of too ready excitability.

Since I have traced so much mischief to the effects of small, but persistently operating, injurious

influences, I have learned to distrust very much what are called alterative doses of mercury. I do not mean to express any doubt as to their occasional great utility; but they require, in my opinion, very much more caution than usually accompanies their administration.

If any medicine, fairly tried, does not produce the effect for which it was designed, it is better to relinquish it; if it act as is desired, and attention is paid to the exclusion of *all injurious influences*, it may be, in general, gradually withdrawn.

In acting on the liver, I usually prefer (if I am using mercury) to give a single dose, that acts gently but decidedly; and then see what a few days' intermission, with a rigid interdiction of all disturbing causes, will do. In my experience, the true effect of medicine on the liver is seldom *curative*, in a direct sense, so much as placing the organ in a *curable* condition. But in this I regard not only the immediate but the *prospective* influence of mercury.

If a patient is seen but seldom, and his liver cannot be trusted without occasional medical excitation, I prefer giving some aperient with a grain of ipecacuanha and eight or ten of taraxacum, to trusting him either with mercury or narcotics.

In many cases, it is of the first importance to make the stomach the portal for medicine as little as possible. In such, occasional injections of mild aperients, or the use of the nitro-muriatic acid baths, are good measures; and these, as well as any measures directed to the skin, as friction, vapour or other baths, are exceedingly assisted by *dry cupping* over the organ; a remedy from which I have, in nume-

rous cases, obtained the most satisfactory results; and which I have seen relieve the liver when all medicine, including mercurial forms, had proved disturbing or inefficient.

There are cases, no doubt, in which the organ continues disturbed and refractory, notwithstanding the most rigid treatment. In my experience, these are patients in whom the assimilating processes are generally defective, and where the management of the liver is only one, though perhaps a leading, feature in the treatment. There is no power to take efficient exercise: you cannot afford the disturbance which medicine of any kind creates; in some, the quantity of medicine taken at various times seems to have had no small share in the mischief. Here I have seen a small seton in the side turn the whole current of affairs in favour of the patient; but it should be so managed as to avoid any reaction. Any attempt to move it about or apply ointment to it, will foil your object:—the system will be disturbed, and you will be obliged to withdraw it. I keep a very small seton needle for the purpose; and, having made it as quickly as possible, confine the seton, so that it shall move about as little as possible, and direct the patient to poultice it the moment it begins to feel troublesome. The idea that counter-irritation here is to do good, in proportion to the excitement or discharge which it produces, is quite erroneous, as it is (with very few exceptions) in regard to counter-irritation in general.

I intended to have related examples of the occasional dependance of many affections of the mucous membranes (especially in women) on disordered

liver, besides those already alluded to, particularly certain severe and, when otherwise treated, intractable forms of Leucorrhœa; but my limits do not allow me to pursue further the numerous other pathological relations of this organ, so influential in the animal œconomy.

The Lungs.

Our knowledge of the real use of these organs is very imperfect, although many offices which they execute are clear and indisputable. In deducing their management from their uses, I shall restrict myself to such points as are already beyond dispute, or capable of being so placed; and to the application of such means as experience shall practically have sanctioned.

That the lungs throw out of the system enormous quantities of charcoal, in combination with oxygen, every one knows; and that the exhalation of these and other matters which together constitute the breath is *warm*, every one knows also. But that the lungs are the generators of heat for the body; that the oxygen of the carbonic acid exhaled is that portion of oxygen taken in the previous inspiration; that these organs obtain their heat by a kind of combination of oxygen and carbon; and many other views, more or less entertained; are notions either *demonstrably false*, or as yet *pure assumptions*. Now, what I am chiefly interested in impressing at present, is the extrication of charcoal, which nobody doubts; but I take this opportunity of announcing in print, for the first time, certain views which I have been for some years developing,

and which I think now placed beyond any reasonable doubt.

The allegation is, that the lungs are refrigerating organs; and that it is by means of them, in combination with the skin, that animals maintain their characteristic temperature, by throwing off superfluous caloric. I contend that this view is susceptible of every thing short of absolute proof; that it explains all phenomena; that it renders intelligible certain facts not intelligible before, and leads us to others, the practical application of which is of great value.

This idea I first announced in a public lecture in 1838; then, in a somewhat more matured form, before the Royal Institution; lastly, in a paper to the Royal Society; but, unfortunately, sent in just before the death of the Duke of Sussex, which, lessening the evenings of that session by two, threw my paper, with some others, upon the last evening; and they were, consequently, read only as to their titles. I hope to find time to publish, in a complete form, the whole facts on which the allegations are founded; but it is necessary that even here I should give the heads of the agrument; because, otherwise, some even of the hints I have alone the opportunity of suggesting might be unintelligible. My attention was originally drawn to the subject by Mr. Maugham, a man of striking talent, and a bold and original thinker, in the following manner. "I wish," said he, "you would consider whether we do not maintain our temperature by the skin throwing off superfluous heat," or words to that effect. I replied that I thought the investigation

would necessarily include the lungs. I will now endeavour to put the reader in possession of such of the facts as I can include in the limits proposed. It is very difficult to represent truly what are the *present views* of the functions of the lungs. The following sketch will, I trust, be tolerably correct and sufficiently comprehensive.

Some believe that the lungs are greatly concerned in generating the heat of the body, and that they affect it by the union of carbon with oxygen, in the manner of ordinary combustion. Opinions originating with Crauford, Lavoisier, &c. Some believe that the lungs generate the heat, but doubt the modus agendi just referred to. Some think the lungs have little to do with the generating the heat of the body, except as conveying oxygen for that purpose. This seems, if I understand the learned Professor correctly, to include the idea of Liebig.

The result of ascertaining that which I believe to be the true functions of the organs, leads, in my view, to the demonstration that animal heat is the result of the *various* chemical changes constantly going on in the body, not at all *exclusively*, as Liebig seems to think, to the play of carbon and oxygen ; but I cannot consider that question now. I would by no means insinuate that the foregoing sketch represents the opinions entertained from the first development of the combustion theory by Crauford, Lavoisier, and others, to the present time ; but I am not aware that any one regards the lungs as simply refrigerating organs, still less that animals owe their *preservation of independent* temperature to the com-

bined action of the lungs and skin, as refrigerating organs, which is the view for which I contend. Nevertheless, it is not absolutely new. Towards the close of the seventeenth century, many persons entertained this idea; and, if I mistake not, it was participated in by no less a distinguished chemist than Mayow.

That the reader may be at once on his guard, I may observe that the whole induction—of which I can only give him a faint sketch—rests on that which, out of the body, is a demonstrable fact, but which, in the body, we will regard as a postulatum: viz. that a warm fluid (blood) brought into practical contact with *cold* air, and both *in motion*, the result must be refrigeration.

The phenomena of wind, fanning, &c. will sufficiently shew that motion indefinitely increases refrigeration.

Cooling will also be greatly accelerated by increase of surface. If I throw a given quantity of fluid into a cooler of a certain size, and it does not cool fast enough, I may hasten it either by employing a larger cooler or throwing in less fluid; still more if I do both. Now, whether I use a cooler, that is, a shallow vessel, and let the air pass over it, or put both fluids in pipes, so that they can act on each other, I do the same thing; and not the least curious part of the matter I am hastening to sketch to the reader is, that in the first lecture in which I announced what I believe to be the true function of the lungs, quo ad temperature, I made a diagram for illustration, which, three years afterwards, I found was actually a practical representation of the patent refrigerating apparatus used in the exten-

sive brewery of Messrs. Young and Bainbridge, at Wandsworth. In selecting facts, I will first take a cold animal, a frog; one whose coldness was referred to the fact that only half his blood was exposed to the supposed combustion. Now, the lungs of the frog are enormous; that is, the surface exposed to air is *very* extensive, and over this is spread only about half his blood; added to which, his skin, a truly breathing organ in *all* animals, is so essentially that in the frog, that it has been proved, by the experiments of Edwards, to be more necessary to life than the lungs themselves. Every one knows that a frog is very tenacious of life; but a smart tap with a twig on the skin kills him instantly, much the more conclusive evidence of the two; amongst other things, it is shewn by the frog that the idea of the carbonic acid expired, deriving its oxygen from that inspired, is an assumption; those animals giving off carbonic acid, when kept out of contact of oxygen, that is, in nitrogen or hydrogen gases, as largely as when they are allowed to breathe it.

It is amusing to see how far assumption misleads. No sooner was it *assumed* that the heat of the body was the result of the union of carbon and oxygen in the lungs, than the frog, really a most capital instance of the contrary, was wrested to support this theory; for, seeing that only half his blood was thrown over an immense surface exposed to air, a process obviously intensely refrigerative—"Ah," they exclaimed, "of course, because only half his blood is exposed to this sort of combustion." Now, a frog is cold because his organization is exposed to intensely refrigerative agencies; and it is conceivable that, his organization and habits in other

respects remaining the same, he would be very uncomfortable if it were otherwise. He is cold, therefore, because he has a small quantity of blood exposed to an immense surface of air; his lungs occupying, when distended with air, the greatest part of his whole body. The other explanation, that he is cold because only half his blood is oxygenated—even had it held good with the frog—is quite inapplicable to hybernating animals, which are many of them perfect breathers, that is, have the whole of their blood exposed to air, as bears, hedgehogs, &c. But it might be said that in these animals absolute cold of climate does that which the more limited combustion (so called) does in the frog, viz. depresses the vital power: but this will not hold; for the torpor is not peculiar to cold climates. It takes place in hot seasons, and in the climate of Madagascar, where the tanrec (a hedgehog) becomes torpid in the hottest weather; and here we arrive at the true relation, viz. that in both cases the animals become torpid when they have no food—excessive cold and excessive heat alike depriving them of insects and of their other means of support. Then we have animals that become torpid in hot *and* cold weather: the snail, for example; but with this remarkable and instructive difference, that his respiration (or, as I should say, his refrigerating apparatus) continues in hot weather, but ceases in the cold. Now, if respiration were a heat-making process, we should here have the anomaly of its ceasing, when most required. I have no opportunity of alluding to the hundreds of experiments made on this subject, or to the not few that I have instituted myself. I will only give one or two. I can in no way ac-

count for the obvious indications having escaped so many experimenters, but by the mind having been prepossessed by the tyranny of a dominant theory.

In the spring, when frogs are invariably shy, I placed in an earthen pan of water, various animals*, frogs, salamanders, and several water beetles; and I gradually raised the water to summer heat, actually to 70 degrees, with the following results, (by Fahrenheit's thermometer)—56 degrees, animals all under the water; at the bottom of the vessel; at 60 degrees, one salamander came to the top, appeared to take a mouthful of air, and again descended. Immediately after this, a dytiscus came to the surface, exposed its tail (its caudal stigma), and again descended. The elevation of another degree or two moved the frogs, who came to the surface, at first again descending; but, by the time the temperature was nearly 70, the frogs and salamanders remained on the surface, and the dytisci, with their tails permanently out of the water. Now, here were animals with two different modes of respiration at least, extending their respiratory power just as the temperature got warm, that is, just as they required refrigeration.

Edwards placed a great number of frogs in limited quantities of water; of course, therefore, with limited quantities of air. In other words, that air only which was contained in the water; and he found that the frogs lived long in proportion as the water was *cold*, provided it was not below the freezing point; but the obvious conclusion, as applied to

* I believe others have tried the same experiment with similar results.

respiration, seems to have escaped him, viz. that as they required less refrigeration, a limited quantity of air went further; that is, supported life longer. Thus—

Temperature of the Water, Fahrenheit.	Mean duration of Life.	Number of Frogs.
At 63 degrees, they lived	1 hour 30 min.	42
60	1 45	
50	5 50	
32	6 7 6, 8 18	
72	1 10 sec.	
90	12 to 32 min.	
108	Died almost immediately.	

Experiments with fish produced similar results.

Fish.

A fish is a cold animal, and it is easy to show that he is a great breather. Like the frog, he is a very good example how truly respiration is commensurate with refrigeration. Water, besides being itself refrigerative, contains about three per cent. of air; but this air is very different from the atmospheric; containing, in fact, nearly double the quantity of oxygen, or thirty-eight per cent. My calculation is, that a fish as big as a man would require very much less water than must necessarily pass the gills in a given time, to be a far greater consumer of oxygen. A trout of half a pound would only require twelve ounces of water in a minute over his gills, to have as much oxygen in proportion as a man; and surely this is a quantity very short of that which must actually pass, when you recollect that a fish lies with his head up stream,

no doubt for his more easy respiration. Humboldt found that, if he placed a fish with every part of his body in water except his head, his skin produced the same changes in the air of the water as when his gills were immersed in it.

This shews the skin to be here again a true respiratory organ. Now, then, when do fish use this organ most? Why, we all know in summer, when they come and bask on the water, in air which allows the coolness of rapid evaporation.

Birds

in general shew a somewhat higher temperature than other animals (though this is a matter that requires revision); and this was said to arise from the quantity of air (oxygen, in fact) to which, by means of their air sacs, and cavities in their bones, they give access. When we come to examine the matter, we find that the various cavities *demonstrably* execute a purpose of great importance to their flight, viz. diminishing their specific gravity; whilst the idea of their being respiratory cavities is a pure *assumption.*

That they are intended for rendering the animal light, is clear, because they are developed in relation with activity of wing; that is, they are most remarkable in birds of prey, in the migratory families, as snipes, woodcocks, &c. and in birds who almost live on the wing, as swallows; and, as Professor Owen once informed me, especially in the humming bird. Again, they are gradually reduced in birds of less activity of wing, until, in the waders, they are

almost lost. In some birds, again, they are confined (as in the ostrich) to the bones where they are most required—an observation made by John Hunter, and confirmed by Mr. Owen, whose kindness at all times in opening his ample stores of information to every inquirer in zoology I have always pleasure in acknowledging.

That a bird requires a liberal supply of heat is very intelligible. He is exposed to refrigerating influences of the most extraordinary kind, to which no other animal makes the slightest approximation. Let any one reflect on air as a *carrier* of heat, on the phenomena of evaporation, on the effect on our bodies of low temperature, if the air have the slightest motion; and then fancy a bird moving in the higher regions of the atmosphere at thirty miles an hour (a moderate speed), why the refrigerating force is almost incalculable. But how beautifully it is met. Here we are not obliged to theorize; the means are as adequate as they are simple. In the first place, all the assimilating processes (those by which the multiform changes of matter, constantly going on, are effected) are extremely active; and which, had we opportunity for going into the question, are plainly enough the sources of heat, though not by the *mere* action of *carbon* and *oxygen*. Now, the air in some of the cavities (the bones, for example) can only be slowly changed; and air, though a good carrier, is a very bad conductor of heat*, as our double windows and many clothes of

* The reader must recollect the distinction. Air is a very bad *conductor* of heat, in the sense in which a metal conducts it from particle to particle; but, in sweeping off that stratum in immediate contact with a heated body, it is in motion a very rapid carrier.

fluffy surface teach us. Lastly, the heat of birds is *kept in* by that well-known worst of conductors, feathers; and these are again aided by that beautiful overlapping of them to the wind, which gives the additional advantage derivable from mechanical arrangement. Then the very small size of their lungs, the restricted motion of the chest from the fixed breast-bone, that the pectoral or wing muscles may have a firm point to act upon, render their respiration (proper) extremely measured. This seems, indeed, to have rendered some express provision necessary; for the capacity of the chest in the direction of the abdomen is conveniently enlarged by the imperfect development of that muscle which, in quadrupeds, divides the chest from the abdomen (the diaphragm); the absence of which has many other interesting relations, into which I cannot enter.

I have no opportunity of saying more in regard to insects than that they are beautiful exemplifications of respiration being a refrigerating process. No animals have such extensive breathing organs: their respiratory tubes ramify all over their bodies: they are, as it were, all lung. Therefore, according to those who would make breathing a heat-generating process, they ought to be very warm; but they are cold animals, seldom more than a degree or two above the surrounding temperature; but *giving off* a great deal of heat, as bee-hives and ant-hills sufficiently demonstrate. They also thicken the argument concerning the use of the air cavities in birds; since there is a similar provision in the *flying* insect in their *extensile* trocheæ.

Wherever we look, whatever animal we exa-

mine, we find evidence, as it appears to me, of respiration being a refrigerating process. Now there are animals allied to the reptile kind, which have both lungs and gills, which, including the skin, is a *treble* apparatus for respiration. But hitherto these are not only inhabitants of *warm climates*, but, with one exception, residents in localities where heat is extricated with *unusual* difficulty, that is, in marshy situations where warmth and moisture are combined; as the siren, the axolotl, and a more recently discovered animal, the lepido siren, found on the banks of the Gambia, of which a most interesting account has been published by Professor Owen. The proteus is a different case, but equally instructive. His habits approximate to those of the frog, if I am rightly informed, except that he lives entirely in the water, and much, near the bottom. Now, in warm weather, the frog puts his skin in requisition in the air, because he cannot cool himself sufficiently in water; the proteus can, because he has gills as well as lungs. The respiration, however, would be much easier than in hot marshy air; and it appears that his gills are not highly developed*. The evidence of respiration being a cooling apparatus, carried on by the lungs and skin, seems to me endless; but it is not necessary to assume that the lungs are nothing else—that may, or may not be; neither is it necessary to speculate *what* part oxgen acts, or nitrogen.

Before we can arrive at the perception of the *entire* functions of the lungs, I suspect that the whole subject will require to be carefully reviewed, with more freedom from the influence of former impressions than it has been hitherto investigated.

* Cuvier.

I will now add a few common phenomena, which I think have been either overlooked or misunderstood, out of a vast number that are happening every day; previously observing that, in the refrigerating apparatus before spoken of, in the brewery of Messrs. Young and Bainbridge, the warm beer, in passing by means of a pipe enclosed in another pipe containing cold water (both fluids being in motion), loses 12 degrees of temperature, when the cold water is 56 degrees, even in one transit of 80 feet.

Common Phenomena.

If the skin and lungs are refrigerating organs, and were both to be embarrassed at the same time, there ought to be an increase of heat. We cannot, of course, institute such a condition; but we see it in fevers, and in some cases very perfectly; and accordingly it is the *only* occasion on which the internal temperature of the body is increased.

When a dog lies panting on a warm summer's day, surely his lungs are not making heat; no, they are cooling him; and these vigorous actions are in harmony with the habits of the animal, who perspires but little.

The panting on walking into cold water is consequent on the functions of the skin being suddenly depressed, and the lungs increasing their duty; but that this is so, and not to make heat to oppose the cold water, is seen in that, before you have been in the water half a minute the panting ceases, whereas it ought to continue, as the cold renders heat more necessary.

When you go into the cold air, you begin by breathing fast; but this again is only until your skin

begins to be active, and then you breathe more slowly.

On the contrary, in high temperatures, both skin and lungs act more frequently and permanently; the respirations, cæteris paribus, being more frequent, and the transpiration by the skin greater. This is wholly irreconcileable with respiration being a *heat-making* process *for* the body.

The most satisfactory experiments have proved that heat is thrown off with more difficulty in warm and moist atmospheres; this is the very atmosphere in which we throw open our coats, and with a long expiration exclaim, how oppressive the weather is!

The vegetable kingdom presents a great variety of phenomena, which, so far as they have any bearing on the question, are greatly in confirmation of respiration being a refrigerating process: of these, I will only mention the falling off of the leaves (the lungs of plants) in winter. And further, that this is, as regards the *tree*, a living process. Now we know, from our green-houses, that *higher* temperatures will not kill plants at this season, and that greater cold would; hence, the retention of leaves would perhaps be injurious or fatal.

Some years ago, I made some enquiries of Mr. Pamplin, a very intelligent nurseryman, at Wandsworth, about the behaviour of evergreens in severe winters; and I found that, of the plants which weathered them with difficulty, those did best which shed their leaves most quickly; and that many that died, did not shed their leaves at all.

Numberless other familiar illustrations might be mentioned, which at the same time point to the true sources of heat; these appearing so multiform

and abundant, in strict conformity with clearly ascertained laws out of the body, even as to have suggested the necessity of refrigerating organs*, if we had not perceived any such provision. There is little doubt, I think, now in the minds of physiologists that the heat of the body is generated in those myriads of conversions of different forms of matter going on in the body—not as due to any special properties of *carbon* and *oxygen*, but those which seem common to all sorts of matter—to which I have already alluded. I am sorry I cannot follow out the evidence of the refrigerating functions of the lungs and skin more fully. The sketch I have given will be plenty for the reader to test the view by. To me the matter appears so certain, that I wonder how any one could have imagined any other result, from cold air and warm blood sweeping rapidly over each other.

In the management of the lungs, the first thing, in common with other organs, will be that equalization of function of which I have spoken; viz. the taking care that the lungs are not called on to do the duty of other organs; and to look to the skin and liver more especially.

To ensure regularity in the liver, I know of no better *general* directions than those already given. If the liver do not perform its functions, and no other disease be established, as hæmorrhage from the rectum, piles, or abscess, &c. the chest will be disturbed either by *too much* blood being trans-

* Could I have detailed the whole facts, in direct proof of the lungs being refrigerating organs, I should still have left an immense mass of facts touching and testing the true sources of heat; for to demonstrate both clearly, they should be considered together.

mitted to the heart through the liver, or *blood* from which bile has not been extracted; and in various manners in different cases, as by palpitations, coughs, difficulty of breathing, faintnesses, &c. including bursting of blood-vessels, of which I have seen very well marked cases from this mode of causation.

I have indeed, not unfrequently, been struck with the little attention which is, in so many pulmonary complaints, paid to the liver, considering the habits of this country, and how notorious the fact, that the lungs are frequently disturbed through this channel.

The few hints which I have to offer on the skin, will be given in the next section, to which I refer the reader.

The influences acting directly on the lungs are, air in its various conditions, and the impurities by which it is occasionally contaminated. With many, perhaps most of these, we are acquainted. Still the atmosphere frequently contains many inappreciable forms of matter; and is occasionally in peculiar conditions, of the real influence of which, on animate and inanimate nature, there is not the smallest doubt, but which we cannot explain. The principle in the odour of flowers which produces so easily, in confined atmospheres, faintness, or that peculiar condition of the air which so suddenly excites the acetic fermentation, are entirely unknown. On the other hand, it seems pretty certain that there are no injurious properties or conditions of the atmosphere which are *unknown* to us, but which may be indefinitely reduced as to their injurious influences by the avoidance of qualities or conditions with which we *are* acquainted, and which

inform us of their presence by their odour, or by other equally unequivocal effects on some of our sensations. The great bulk of the atmosphere is, we know, formed of about four-fifths nitrogen and one-fifth oxygen, with a very considerable *quantity*, though a very small *proportion*, less than one part in 100, of carbonic acid. Besides this, it is capable of holding a considerable quantity of moisture in solution; this capacity being (cæteris paribus) increased by its warmth, as we see by the readiness with which it deposits it on a glass of very cold water. Then, again, air is a very different thing as it is still, or in motion. Now, the foregoing are what I may call the more *essential* properties of the atmosphere: they are all those which we can most easily ascertain, or which we can most readily control and adapt to our particular conditions. It is a very beautiful arrangement, also, by which most injurious gases are either refused admission to the lungs, as carbonic acid (when unmixed), or create unpleasant sensations when it is diluted; or announce themselves by their disagreeable odour, as sulphuretted hydrogen, muriatic acid gas, chlorine, &c. Comparatively simple, however, as the more essential qualities of the atmosphere appear to be, yet it is obvious that they admit of great varieties of condition; that is, the air may be more or less warm, more or less moist, lighter or heavier, or more or less in motion. All of which conditions, with the several varieties of combination, of which they are clearly susceptible, constitute very essential differences in their principal relation to heated bodies. I mean as carrying off heat. These varieties, and their respective fitness for the body, are, however, beautifully sug-

gested by our sensations. A dry atmosphere is unpleasant to us; a warm and moist one oppressive, because it carries off heat with difficulty; a cold and moist atmosphere carries off heat rapidly; and we feel it colder than the thermometer expresses. Medium temperature is most agreeable; a breeze is delicious; we soon get tired of the gale: the one carries off heat in proportion to its increasing development by exercise; to meet the other, we either get cold, or are obliged to exalt exercise into fatigue.

In short, our sensations are exquisitely good monitors, if we would only attend to them, in conformity with the suggestions of common sense. It is difficult, however, to get people to think; and, when the weather is inclement, instead of taking exercise, with efficiently protecting clothing, they sit in the house, and content themselves with dignifying what, nine times in ten, is mere indolence, by calling it the suggestions of nature.

Few people can choose the soil or the locality in which they live; but most people can accomplish good drainage; and I sincerely hope that our Government will hasten to accomplish it for all. Efficient drainage is not only necessary to free the air from the most poisonous gases, as sulphuretted hydrogen, &c. but it has the additional advantage of rendering many objectionable soils healthy. The air of crowded cities, laden with smoke, every one knows to be undesirable: it always introduces matter to the lungs with which they ought not to be called on to deal; and in tumours, or any other serious diseases, should be avoided if possible.

I am here, of course, not speaking of diseased lungs: but of the management of comparatively heal-

thy organs; yet I cannot help being surprised, with my experience on the effect of correcting and equalizing function, that such immensely rich hospitals as Guy's, St. Thomas's, and St. Bartholomew's, should have no establishment in the purer air of the country, no means, either with a view to the correction of disease, or improvement of science, of trying that remedy (pure air) which we recommend to our private patients every day. Nor can I think, acceptable as every plan in any shape is, which practically holds out relief to our suffering brethren, that, in the present state of science, even the broad mantle of Charity quite covers the defect of building hospitals for diseases of the lungs in the imediate precints of the largest and most smoky city in Europe. I know there are great difficulties in the way of effectually carrying out an inductive medical science; let it be granted; but, until we can command conditions which are demonstrably reasonable, let us not, in a climate so variable, and with habits so obnoxious to liver-affections, sit down and say that consumption or even cancer are incurable, until we have *decided* that the cure of diseases renders the exclusion of *only a portion of* disturbing agencies necessary.

The management of the air, by the institution of special conditions by means of particular temperatures, the use of respirators, &c. hardly comes within the scope of such management of the lungs as is likely to be necessary in tumours, or in surgical diseases. Most of these means are good in their way, and often of great service, so long as they do not induce a neglect of such other treatment, or attention to the other functions, as is always necessary. The respirator, for example, when properly used, will

enable a patient to take exercise in an atmosphere which would be probably impracticable without it, but like other good things it may be abused, as when one of unnecessary power is used, or when the relief it gives induces the neglect of other measures. The management of the lungs in tumours will be chiefly in securing a good and moderately poised quality of atmosphere, free from any injurious peculiarity; and distributing the functions equally to their respective organs.

The sketch which I have given of what I believe to be the true functions of the lungs, leaves me no space for further remarks. I may observe, however, in conclusion, that the improved treatment of pulmonary affections of horses is very much in favor of my views. Mr. Sewell, the distinguished Professor of veterinary surgery at the College, tells me that *now*, in all inflammatory affections of the chest, they put the horse in the most open and coolest stall they have; that they have built some on purpose, and the horses are placed there in moderate clothing, even in frosty weather. That it is in vain to combat severe pneumonia, if you have to contend with hot stables; but that, with the cool atmosphere, one good bleeding will frequently suffice. Now, according to my views of the lungs, I should say, yes; you meet the disease by relieving the organs, so far as that is possible, both directly and indirectly; Directly, by first leaving the refrigerating surface, so far as you can, the same, whilst you increase the refrigerating power of the air, by lowering its temperature, and diminishing the quantity of the fluid (the blood) to be refrigerated; Indirectly, by encouraging the skin*, the natural compensating

* Mr. Sewell said they avoid *checking* the skin by clothing.

organ to the lungs, as engaged in a concurrent function.

This is all in harmony too with the notorious unwholesomeness of hot, crowded rooms, which are further pernicious in converting the compensating activity of the skin into an element of future mischief; for so soon as the transition to a cold atmosphere occurs, the skin is depressed, and now a sudden increased requisition is established on the lungs. Thus people catch cold, as it is termed; but, as I have formerly explained* the rationale of this fruitful parent of disease in this country, I must now proceed to offer a very few hints on the skin.

The Skin.

In all climates, that organ, through which the system is connected with the atmosphere by so extensive a surface, must necessarily exert a very commanding influence on the general condition of the body; and accordingly we perceive a beauty of adaptation in the skin of all animals to their external relations, in the highest degree interesting and instructive. As the skin is a refrigerating organ, we see it naturally clad in all animals with substances which conduct heat more or less rapidly in very obvious relation with the climate and season in which they are placed. In cold regions, the skin is always covered with bad conductors of heat (as fur); and as the climate becomes more and more temperate or warm, we gradually get the fur assuming more and more the character of hair; that again becoming more fine and sleek, until we arrive at the comparatively bare covering of the elephant

* See "Unity of the Body."

and many other tropical animals. In animals of prey, we still have a considerable coating of hair, which assorts well with their predatory habits, and the cold tropical nights in which they are for the most part exercised.

The adaptation of covering is also seen in the thickened coats of animals in winter, in the peculiarly heat-confining covering of birds exposed to air in rapid motion; and again in the sleek coats of highly groomed and clothed horses, the winter coat disappearing in proportion as an energetic condition of skin and the adoption of artificial clothing removes the necessity for it.

We further observe, that, in nature, the importance of warmth to young animals is every where recognized; sometimes in the immediate covering given to the young, as in the lamb; sometimes, by being made an engrossing object of parental instinct, as in the nests of birds, of many insects, and the prevalence of similar provisions in quadrupeds.

Of the striking cleanliness of animals, who are more or less in a state of nature, I have already spoken.

These hints, which convey a very imperfect idea of the real refinements of adaptation in nature, are sufficient to shew that we may learn from simple observation what the most extended experience only comfirms; viz. that the principal points in the management of the skin are cleanliness, and that adaptation of it to the atmosphere which shall, in *conjunction with the vital relations of the skin to other organs*, regulate the quantity of heat extricated from it in the atmosphere, by the interposition of non-conducting substances, which detain or liberate the

heat in harmony with the climate or other relations of the animal. These apparently simple requisitions are, in practice, often obtained with difficulty. If fashion favour necessary clothing, well; if not, the difficulty may be insuperable.

In very hot and in very cold regions, mankind clothe themselves with considerable success in adaptation to temperature. In our generally moderate, but *extremely variable* climate, little pains appear to be taken, where unquestionably they are most required.

Many persons make little difference in summer and winter. Young people are seldom sufficiently clad; that of which poverty bereaves the poor, vanity or fashion deprives the rich; and in females of all ages, amongst the young, we observe defective protection exactly at such times and of such districts as most favour attacks of disease. Ladies may be seen in the morning sensibly clad, in temperate and well-ventilated rooms. In the evening, with large districts of skin exposed, including those beneath which lie such parts of the lungs as are most commonly diseased; and with the feet, where warmth is most salutary, clothed in silk as thin as gossamer. There is no doubt that a popular diffusion of the knowledge of the general laws by which the system is governed would save a great many lives; but I question very much whether a larger number of young females are not sacrificed to deficient knowledge of the exposure of the skin, than to any *single* source whatever. Many persons make a difficulty in wearing flannel, where that protection is required, on account of the irritation it so commonly at first produces; but, to say nothing of that being, in some instances, to a

certain extent desirable, I have never found any difficulty in graduating a patient into efficient flannel by beginning with the finer sorts (or even real cachemere, if necessary) which are sold by our first-rate hosiers. Warmth then, and due protection, in our variable climate, are at all times beneficial; but they are never to be neglected in the treatment of tumours, for other reasons.

Cleanliness is essential to the healthy state of the skin. This is sometimes a difficult thing to obtain. When you direct people how to proceed, many will exclaim, "What, wash the whole body every day?" as if you were about to establish some very strange and unreasonable requisition. They seem to have little idea that this is necessary to doing the organ justice, still less that a little habit will render it a real luxury. There is nothing in all animals so certainly prejudicial as retaining *any form* of matter which any organ has separated.

Cleanliness, however, in animals who lead an artificial life is not all; a certain energy of cutaneous circulation is necessary, not only to a due reaction against the effects of cold, but in maintaining that balance in the circulation which is so valuable. To effect this, exercise is the natural stimulant. But as a thousand considerations obstruct the perfect attainment of this remedy, so it becomes, in a vast number of cases, necessary to supply a substitute, and one of these is active friction. We see how it affects horses and other domestic animals; and we very easily ascertain its influence in ourselves.

The extraordinary effects of these simple measures—I mean cleanliness and friction of the skin—have been observed at all times; and, accordingly

we find their real properties not unfrequently masked by medicated forms and peculiar nomenclature. They thus become a famous field for the operation of one kind of quackery.

Establishments are formed where the real virtues of rigorous and efficient friction are attributed to peculiar instruments or special modes of administration, and where the *generally* powerful agencies of warmth and moisture are merged in the more *occasional* efficacy of medicated vapours.

Hydropathy is an offset from this system of things; and, like many practices containing a great deal that is erroneous and mischievous, obtains that credence, which so often results from the smallest admixture of truth. That, in its powerful excitation of the skin (where that happens), it does good occasionally, can hardly be doubted; but the objection is, that, in adopting measures attended with very considerable risk—especially if there be any unsound organ—it at best accomplishes that only, which, if people would consent to take the same pains, may be accomplished without any risk at all.

Most of these systems (so called) include a plain and careful diet; and this is carried out, in establishments specially devoted to the scheme, with a regularity that not ten people in a hundred will allow you to effect whilst living in general society.

Hydropathy, in its recognition of regularity of life and activity of skin, has truth. In its general and indiscriminate application, as well as the *mode* in which it effects its appeals to the surface,—and again, in its neglect of the special conditions of other organs, it is abundantly fraught with mischief, ignorance, and error.

The essentials to the skin are, in various degrees, friction, warmth, moisture, cleanliness; to these may be added the invigorating influence of the tepid or cold baths: and if these be employed with due regard to the special condition of the skin, that of other organs, and the peculiarities of the individual, they become powerful agents.

Whilst I would thus endeavour to point out the general character of these sorts of application, I would by no means be understood as implying that the skin may not, and is not, daily made the portal for the introduction of medical agencies; still less impugning the convenience of such establishments as Dr. Green's, in Marlborough-street, and many others. I only mean to say that most of the class of applications to which I have been alluding owe their efficacy to their effecting, in combination with friction, cleanliness of surface, and activity in its circulation. The principles are the things to be borne in mind; because, these being understood, we are seldom at a loss any where; so that, in a poor man's cottage, a kettle vapour-bath and a vigorous rubbing from the rough hand of a kind-hearted neighbour, may be occasionally no bad substitutes for shampooing and Brighton. I would observe that, when you desire to prescribe the cold-bath, and have doubts about the reaction, it is a good plan to introduce it by half-a-dozen shower-baths, beginning warm, and gradually reducing the temperature.

In applying friction, I generally recommend brushes; because they can be more readily adapted to the varying sensibility of the skin. Those I use have a web-strap for the hands; but, in many

cases, to get the rubbing efficiently done, it is necessary to have an attendant for that purpose.

I cannot here repeat what I have formerly said, in other works, on the extreme importance of the skin in all diseases. Whether it be regarded with reference to its relation to the atmosphere, to the various matters it separates from the body, the security to other organs from the activity of its circulation, or the vivid and instructive sympathy it has with every function of the body, it alike strikes the practical observer as an organ which is of extreme interest—one that always claims our earnest attention, notwithstanding that, even yet, it is extremely neglected in this country.

The skin is occasionally acted on very beneficially by medicine. The more common and efficacious are ipecacuanha and antimony. Camphor I find also often serviceable. These are the medicines I more commonly employ; but I seldom act on the skin *only;* and I prefer (where it is not contraindicated) the vapour-bath, which the little kettle I have directed Mr. Livermore, of Oxford-street, to make, enables me to prescribe any where with expedition and efficiency.

The explanation of the general utility of activity of surface is easy. If the circulation be active at the surface, it necessarily opposes congestion within; and thus, in more or less degree, is a remedy offering much assistance in almost all diseases, attended as they are with disturbance of equilibrium in the circulation.

In fact, I know not that any harm would result, if, in every disease, liberal ablution and friction,

applied in a temperature and adapted to the peculiarities of the case, were a sort of habitual prescription;—were it not, that its very comfort and efficacy has a tendency to lead to an *exclusive* reliance on it.

In Tumours, which are but mere symptoms of deranged function in some one or other of the organs, the skin inclusively, the regulation of the surface is always a matter of great importance. If you are in difficulty as to the primary organ, activity of surface is sure to assist you—sometimes by its direct effect, sometimes indirectly in its sympathetic influences on other organs; whilst, if the skin has been previously disturbed, you are adopting emphatically the course desired.

These few hints convey, I am aware, very imperfect directions; but my limits do not allow me to extend them.

The Bowels.

Torpid bowels are the greatest plague of many persons' existence, unless it be the anxiety to correct that condition.

No doubt, regularity of the bowels is one of the signs of health, and a great security against disease; because this regularity *usually* implies a condition (*proximately* healthy) of many other organs. Strange as it may appear, the cause of torpid bowels is comparatively seldom in the bowels themselves, otherwise than proximately; of course, they have their primary affections; but their torpor generally arises from other organs; and here we get a

sort of clue to the reason *why* habitual torpor of bowels is never corrected by purgatives. Imperfect digestion, slow digestion, torpid liver, unhealthy condition of skin, undue activity of kidney, various conditions of mind, are all causes, contemporaneous occurrences, or sequences, of costive bowels. I make these distinctions because I have been very often unable to determine the order of occurrence; but this I know, that the correction of the contemporaneously disordered function very often proves to be the thing needful in restoring regularity to the bowels. No cases are nearly so difficult as those in persons who have for a long time habituated themselves to the use of aperients, to obviate the effects of sedentary habits.

In all cases, I endeavour to ascertain with what organs the bowels hold the most lively sympathy, because slight impressions made on that organ are much more effectual and permanent in their effect than any direct stimulation.

In the same manner I examine the history, with a view to develop the fact whether the torpor has been a primary or secondary impression. The liver, of course, is frequently in fault; but of all the organs in the body, I have seldom found any, except the liver, afford more recompense for special care than the skin; and, in many a case, measures directed to the surface have restored regularity of bowels when all else had failed. I need scarcely observe, that whatever the special plan may be, it should be accompanied by an appropriate diet, and attention to other organs.

When bowels have become torpid under habitual *medical excitement*, deficient exercise, or di-

rectly sedentary habits, the liver is almost always implicated. In cases of this class, exercise is essential. In the medical treatment, it is desirable to proceed as you would with a dram-drinker's stomach; that is, as you would wean his stomach from alcohol, so must you the bowels from factitious excitement. You give *small doses* of aperients until the desired effect is produced. You then *gradually* withdraw your excitant, whilst you increase exercise, adding at the same time any impression which the case may have suggested on other organs. Here it is that sudorifics are often so beneficial. These cases, however, are frequently tedious; but, in tumours, the accomplishment of reasonable exercise and the reduction of factitious stimulation are essential. Sometimes an opposite condition of bowels prevails. I mean habitual relaxation. I had lately under my care a lady in whom the bowels were habitually relaxed, apparently coexisting with deficient action of the liver. This is a condition of bowel often very difficult to correct. The cases which I have seen of it have appeared to depend on three modes of causation—deficient action of surface; torpid liver, to which the secretion from the bowels seems an attempt at compensation; or from a diet unfitted to the case. That state of immoderate action of the bowels, from too much bile, is altogether a different case. The condition I have been referring to is an habitual laxity of bowels, just enough to be troublesome, without amounting to diarrhœa; more, in fact, like that condition which sometimes accompanies stricture and other affections of the lower bowel. When we recollect that the bowels have a serous and muscular coat, and are

lined by a mucous coat, we may readily perceive how extensive their sympathies must be, and that any of them may represent the cause in a particular case. We, therefore, not only understand why it is so futile to rely on purgatives for the permanent correction of torpid bowels, but also why, in the opposite or relaxed state, it may be unsafe to prescribe chalk, or the medicines by which it is so often accompanied, too indiscriminately.

Some cases appear to depend on a want of condition in the muscular coat of the bowels; and a teasing state of torpor is relieved by a more generous diet; and in such a case I have seen bark very serviceable. If you get a very obstinate case, and the bowels have been habitually subjected to factitious stimulation, it is by no means a bad plan, if it be adopted with the necessary caution, to allow the bowels to go until they *do* act, watching every symptom which may arise, and promptly administering your purgative, if necessary; taking care, of course, to have the diet all the time as unobjectionable as possible.

The causes of torpid bowels are so numerous, that no cases require more circumspection, in order to ascertain the conditions on which they depend. But if this circumspection be vigilant, it is generally successful. Without this, success is out of the question.

In some cases of torpor, the bowels are tertiarily affected, and in many different ways. In one form, irritability of the kidney disturbs the skin, and that organ the bowels; in others, the irritable kidney is the *effect* of costive bowels; but these and many other complicated varieties and transposition of

cause and effect become very simple, when the history of cases is *fairly* examined, and the views thence obtained carefully *tested* by the treatment.

As the bile is the natural aperient, if I may so express myself, the liver should, of course, always have special attention.

The evacuations should be examined, both with a view to determine, so far as they enable us to do, the activity of the liver, and also to inform us what aliment (if any) be undigested, as this may be of material use, not only in relation to the bowels, but other organs; since the omission of any article undigested will be certain improvement in the diet. Many a disordered alimentary canal have I seen corrected by this source of information alone.

These inspections are further necessary in determining the presence of unusual accumulations, some of which form a very extraordinary class of cases. They are presented to us sometimes in such deceptive forms, that it is almost impossible for a man to be sufficiently on his guard, until he has had some experience in regard to them. I have met with several, and have seen them mistaken for tumours and also for herniæ, under circumstances which should render us very charitable in judging of those who have such cases submitted to them for the first time.

I will briefly relate one, to put the young surgeon on his guard. I was consulted in the case of a young widow lady, concerning (as alleged) a tumour of the abdomen, which was reported to have existed some months, and for which various means had been employed. As it had been at times very painful, it had been leeched and poulticed, from which she said she had experienced *decided relief.*

When I examined the abdomen, surely enough there was a firm body reaching from just below the cartilages of the ribs, on the left side, and traceable, in a mesio-lateral direction, towards the lower part of the abdomen, where it appeared to be lost in the region of the uterus. The substance seemed to be within the abdomen, and gave no indication of fluctuation. I believe that the idea entertained had been that the tumour was probably the left ovary; and the lady's account lent little assistance in determining this point; on the contrary, some disturbance in the uterine functions rather favoured that idea. The tumour was represented as having commenced when her mind was a good deal disturbed by affliction (the death of her husband), which had happened some months before. Not to detain the reader unnecessarily, I may observe that a cautious examination of the case induced me to observe, to the gentleman with whom I was in consultation, that I had seen such curious cases of loaded bowels simulating other diseases, that now, in deciding on the nature of more or less circumscribed and equivocal intro-abdominal tumours, I was in the habit of first carefully emptying the bowels, which I recommended to be done in the present case; not as delivering any opinion, but as obtaining the conditions necessary thereto. This was accordingly done, in conformity with my usual plan for that purpose, which is to give very small doses of any mild, but certain aperient medicine; so that about thirty-six hours may elapse before the action commences; and this I usually find effected with considerable success by half-grain doses of aloes.

She accordingly proceeded with the treatment pro-

posed: the second day the bowels began to act; and an immense evacuation of fœcal matter occurred, with entire disappearance of the supposed *tumour*. I could mention several similar cases, and also where the (so called) tumours have simulated hernia. The tumour has attracted the attention of the patients immediately after a fall; and when you arrive, you are told that having fallen, a pain was felt as if something "had given way;" when, on examination, a swelling is discovered, which is painful, &c. You examine, and you find a swelling, not hernia, but still having, in some instances, characters for which I have known a very eminent surgeon recommend a truss; but, suspecting the case, you empty the bowels, and instantly the tumour vanishes. I could mention many other cases similar to the two I have just noticed; but perhaps I have said enough to impress the practical inferences; viz. 1st. that such cases are by no means uncommon; and 2ndly. that, if you desire to empty the bowels so as to ascertain that the torpor is not the result of defective power, diminished sensibility, or other results of accumulation, there is no other way of securing the object than by a most gradual excitation, assisted, if necessary, by injection; but without, if possible. Deficient space obliges me to omit a number of hints as to numerous manœuvres necessary in different cases of torpid bowels. I will only add, therefore, that, in certain cases, most satisfactory results follow a judicious variation of the food. I do not mean variety in a single meal, but on different days. We like our dinners to be varied; with most of us our morning meal is the same every day. Breakfast, therefore, is most convenient for the adoption of the hint

here intended. I often recommend patients to change their breakfast daily; which may be done without any difficulty, by tea, coffee, cocoa, milk and water, kettle broth, hot toast and water (very good, with milk and sugar), bread, maccaroni, butter, bacon, scraped meat, and many other articles. When you have previously determined the diet, these changes offer the advantage, that the very article of which you prohibit the habitual use, taken once a week, often becomes a *successful* stimulant of secretion from an organ which in its daily use it disturbs, *because* it excites without producing secretion; as bacon, butter, and greasy matters, in relation to the liver. I must, however, hasten to the remaining matters; only stating, in conclusion, that whilst nothing is more to be desired than regular bowels, it is a great mistake to suppose that such condition can be really represented by the factitious stimulation of purgatives; and for the same reasons that render so much of medical practice ineffectual—that, in the majority of cases, effects are acted on, instead of causes.

Kidney.

The kidney may be regarded as the great sewer of the animal œconomy, wherein a number of heterogeneous matters are, by a most wonderful chemistry, dissolved, and held in solution until they are discharged from the body.

As, therefore, the matter dissolved in the urine must depend on what is *taken into* the body, and on the condition of other organs, with which the kidney has either ordinarily or occasionally a con-

current function, we should expect that the urine would be, as the chemist finds it, a very complicated secretion.

The kidney is an organ very slow in becoming diseased. Accustomed all my life to see a good deal of disease of the urinary organs, I have been surprised to observe how long the kidney is sometimes exposed to disturbing causes with impunity; both from derangement of the digestive organs, on the one hand, and the bladder and other parts of the expelling apparatus on the other.

The function of the kidneys is so important, that they are guarded by the most vivid sympathies, the conservative tendencies of which are very apparent; especially in the brain and stomach. If there be any absolute obstruction to their secretion, there is sickness, &c. the stomach is unable to continue those primary assimilative processes requiring, prospectively, the action of the kidney; and the brain soon becomes disturbed, so that the energy necessary for the general conversion of matter in the body is also indefinitely diminished.

Such interruptions, however, at all approaching to suppression of its secretion, are either temporary, as in the passing of calculi from the kidney to the bladder, or extremely rare.

The kidney is also a beautiful illustration of the principle, that the symptoms or even seats (so called) of diseases are seldom the seats of their causes. It exemplifies very impressively how ignorance or disregard of this important distinction may indefinitely retard the successful treatment of disease. I allude to the history of diabetes.

I believe it was Dr. Rollo who first drew par-

ticular attention to the diet in this fatal malady; after which, Mr. Cruickshank, the late Dr. Latham, and others, investigated the subject; but it is to the position taken by Dr. Prout that I think the progress in our knowledge will be chiefly owing, who regards diabetes as the result of disorder in the digestive or other assimilating processes. That this, again, may in some cases depend on or be connected with other organs, not strictly assimilating, is highly probable; the skin, for example; but that Dr. Prout's view is in the main correct, I have no doubt; and, when an increasing conviction of the necessity of a tenfold greater particularity in the adaptation of the diet to the special states of organs shall have led to its practical adoption, that diabetes will be cured as readily as it now appears in many cases to be indefinitely controlled by the adoption of those principles which Dr. Prout has recommended.

The extreme complexity of the composition of the urine puts perfect analysis out of the question, in the daily duties of the practical surgeon; to say nothing of the time, chemical skill, and opportunities which it requires. Particular cases may be subjected to the analytical chemist; but a good deal of highly useful information may be acquired by comparatively simple means, and a rigid observance of the condition of other organs. The specific gravity of the urine, with the average quantity, in a general case, will afford a good indication of the general amount of work which the kidney is doing. The quantity of urine will vary of course, being more in winter than summer; and so will its specific gravity in different parts of the day. The ordinary quantity is, perhaps, about thirty to forty ounces.

Specific gravity is from ten to twenty-five parts in a thousand heavier than water. Any thing beyond this is generally indicative of something more being thrown on the kidney than characterizes a perfectly healthy distribution of function. But temporary variations arise; and it is not easy to draw the line between what is to be regarded as a more or less usual and a more or less disturbed condition. Some harm, too, is done by refining unnecessarily. The main thing to recollect is, that the kidney itself is seldom really at fault; and the disturbed appearances generally result from some improper matter having been introduced into the system, or from some organ, which is defective in its duty, having necessitated unusual requisitions on the kidney. In testing urine and facilitating an examination of its deposits, a tabular digest of Dr. Griffith's excellent little book, and his papers in The Medical Gazette, would, I should think, be found useful. I have not yet seen the recent work of Dr. Golding Bird; but from the attention which I know he has paid to the subject, it can scarcely be other than an acceptable assistance. In attempting to correct any disordered condition of the urine, a narrow inspection of the diet, as adapted to the actual power of the stomach, liver, or any other of the assimilating organs, is now agreed, I think, on all hands to be the paramount consideration. Because, should the disorder be the result of defective assimilation, no other cause will be successful; and if it should be the result of the torpor of some other organ, as the skin, the adjustment of an easily digestible diet will be an essential assistant. Excessive secretion of the kidney is frequently corrected with imperfect action of the

skin, and is in fact a diseased exemplification of that healthy capacity of interchange of function which exists between the two organs. Exaggerated depression of cutaneous action will produce severe forms of disturbance of the kidney and other urinary organs. I recollect a boy, about thirteen years of age, who for four years had been subject not only to an undue secretion of urine, but the distressing concomitant of not being able to retain it. Examining his functions, I could discover little wrong, except his skin, which appeared to have been sensitive and torpid, since the occurrence of hooping-cough; and, therefore, I confined my remedies to friction, bread and meat diet, and ipecacuanha and camphor, in small, occasional doses. I do not mean that his other organs were not guarded from obvious sources of disturbance; but the specially treated organ was the skin. The activity of the organ was gradually restored; and the disturbance of the kidney, as well as the incontinence, disappeared. Sometimes, on the contrary, the kidney will be sluggish; and, provided there be no suspicion of disease, mild diuretics are at first often serviceable; but I never continue them. In the same limited manner, I prescribe diuretics, sometimes when my ultimate object is to restore deficient function of the skin; temporarily to augment the compensating action of the kidney, whilst I am correcting those functions, on disorder of which the state of the skin is supposed to depend, or whilst administering to the skin directly, when it is supposed to be a primarily affected organ. In neither case do I continue the diuretic more than a very limited time; *generally* a week at farthest.

In a former work*, I have shewn that, in affections of the skin, bowels, &c. the kidney may, like other organs, be made frequently the channel for advantageous impression, as in acting on torpid bowels, and as in the case wherein an impression on a torpid kidney was followed by the healing of an ulcerated surface of the whole leg in an *old person* in a single week.

In various states of indigestion, acids and a variety of other principles probably are generated, highly disturbing to the kidney; and, as Dr. Prout has sufficiently shewn, dangerous, in the tendency to produce calculous and other depositions. Certainly, if you can prevent acid, for example, from going further than the stomach, it is an advantage; for it may in some modified form get into the circulation, variously disturb the œconomy or the kidney in effecting its ejection; and, carefully administered, alkalies are, in this way, occasionally useful. At the same time, it is difficult to over-rate the mischief which a certain *abuse* of alkalies is capable of producing. The habitual use of them to correct acidity of the stomach is seized on by patients as a preferable remedy to that unwelcome circumspection as to diet, which is the real desideratum; and in this way, not only is the digestive power sometimes injured, but a state of kidney induced extremely favorable to deposition. There is no solvent comparable to healthy urine; and that is alone to be obtained by a kidney to which the assimilating processes bear a healthy relation. The habitual use of alkalies to neutralize acidity in no way affects the

* "Unity of the Body," p. 181.

cause of the evil. The real desideratum is, not to neutralize the acid so much as to prevent its formation; and to this end, all the alkalies in the world are more than useless; for not only do they not *prevent* the function of acid, but they very frequently harm the quantity secreted.

In many cases, even where acids are formed in the stomach, the mineral acids are very serviceable, because they improve the digestive power of the stomach. Whether they do it by a direct effect on the stomach or by stimulating the liver, I know not; but of the fact I am certain. Nevertheless, after all, we can hardly be too cautious in prescribing either acids or alkalies where we have reason to suspect any irregularity in the kidney. It is extremely easy to do harm in introducing any matters into the system which may interfere with or embarrass the healthy chemistry of the kidney; whilst nothing but good can possibly result from adaptation of diet to the digestive powers. This is the real source of relief to the kidney, whether we consider the effete matter left by digestion, or that arising from the destructive assimilation of the various parts of the body, consequent on that renewal which they are constantly undergoing.

Many years ago, in writing on strictures of the urethra, I impressed the frequency with which the general health and kidney were disturbed by strictures and other diseases of the urinary passages. The condition of those parts should, therefore, always be investigated, and their prompt correction a primary consideration.

Although the kidney is disturbed by all organs in different cases,—sometimes by having duties im-

posed on it, in compensation for torpor elsewhere, sometimes by sympathy, as from mental disturbance, —yet, after the digestive organs, I have never observed any beneficial influences conveyed to the kidney by impressions on any other organs nearly so marked or uniform, as through the promotion of a vigorous condition of the skin. In affections of the bladder and urethra, it has been equally remarkable. I have seen a highly irritable bladder discharging quantities of mucus, and which had been kept in check very imperfectly by active doses of calomel and opium, yield entirely to friction of the skin, and three effectual administrations of the vapour-bath. Although, therefore, I would most earnestly recommend a careful examination of the kidney and its secretion, and especially its relative activity to other organs, and although I admit the occasional excellence of carefully-prescribed medicines, of alkaline, acid, mucilaginous, diuretic, sedative, or other character, in cases respectively suggesting them; yet I cannot but think that many will act most safely in confining themselves to gentle excitants (diuretics), where the kidney seems torpid; and, where it is doing too much labour, to rest attempts at its *permanent* relief on a rigid study of the defective points in the assimilating processes; and in the mean time relieve it by temporarily rousing the surface into energetic action; by which I do not so much mean profuse perspiration (though that is, in reason, good also) as a steady and sustained freedom in the insensible perspiration and permanent vascularity of the organ.

My limits oblige me to close the few hints on which I have ventured in regard to the kidney, as

sketching the practice I usually pursue. The numerous sympathies of this organ, to be gathered only from the facts of surgery, *in conjunction* with those shown by the diseases more generally presented to the physician, as unfolded by Drs. Bright, Prout, and others, are the true sources whence the safe and successful management of this organ is deducible.

CONCLUDING HINTS ON ORGANS.

Pancreas, Spleen, Genital Organs.

In the foregoing hints on the management of organs, it has been more my object to *sketch* my own practice than to presume to dictate to others; and I have confined my remarks (few as they are) chiefly to those organs of whose functions we have some degree of well-ascertained power.

With regard to the pancreas we know very little, and of the spleen still less; yet, in regard to the latter, we can exert considerable influence.

We see them both developed in the animal kingdom, as extensions of the assimilating apparatus; and there can be little question but that the reasonable mode of relieving them from disturbance is to secure a diet adapted as accurately as possible to the organs which are engaged in the primary stages of assimilation; which either precede, or which are contemporaneous with, any accessory function which may be exerted by the pancreas or spleen. These organs are, of course, the masticatory apparatus, the stomach, its continuation, the duodenum,

and the liver. It is *possible* that the state of the salivary system would be some guide to the condition of the pancreas. I know of nothing positive on this point; and the pancreas is too seldom diseased to enable us to educe any thing decided, from that source, as to the precise nature of its functions.

Although we are perfectly ignorant of the real functions of the spleen, and although the various and sometimes absurd speculations as to its use may well discourage any guess-work; yet, in the human subject, when we see the great splenic vein contributing so largely as it does to form the enormous vena portæ, whence the bile is secreted, we may be quite certain that the vascular condition of the two organs will reciprocally affect each other. There can be no doubt whatever that in many cases, wherein the symptoms point very decidedly to the spleen, the most perfect relief is obtained by increasing the secretions of the liver. Indeed, their anatomical relations alone render this almost necessarily so. And, again, there are cases in which both your original opinion and the results of your treatment seemed alike to point to the liver, yet where the seat of disease turns out to have been the spleen.

I saw a lady, not long ago, in whom I suspect that the spleen is affected; though it was only after having seen her many times that, with all my enquiries, I had obtained any evidence as to the spleen. At first I could not find that she had ever had any illness at all; but circumstances inducing me to suspect the spleen, I asked her if she had ever had ague, when I found that she had formerly suffered from it very severely.

In the human subject, the extensile tissue of the spleen—whatever other office it may perform—must certainly be often a useful safety-valve, as it were, in congested states of the liver; and there is nothing whatever in the whole history of agues which, to say the very least of it, in any degree militates against the idea that the enlargement of the spleen, so common after severe and repeated attacks of that complaint, may be deposition,—the result of blood obstructed, as to its free return, by congested or otherwise disordered liver. Notwithstanding these relations, which are very practical in their bearings, I find, from Mr. Owen, that, in his extended examinations of the animal kingdom, he finds the spleen developed much more in relation or proportion to the pancreas than to the liver. Perhaps they *both* constitute *one* relation to the liver, which certainly would accord very well, *in the human subject*, with the situation into which the pancreas pours its secretion, and those anatomical relations of the spleen to which I have alluded.

In all cases of organs with whose functions you are unacquainted, a *careful* regulation of activity of skin should never be neglected.

The Heart

I have little to remark on. The nature of its functions renders it almost never a *primarily* affected organ; yet it sometimes presents itself in a physical sense as such, from the readiness with which it becomes affected, by enduring affliction or other long-sustained or depressing mental impressions.

Notwithstanding the prevalence of this idea from Hippocrates downwards,—my own observation leads me to doubt whether even the influence of the mind in these cases is not in *great part* due to the previously disturbed state of the bodily organs; affliction being the last coup, that renders the previous disorder apparent.

In the management of the heart, the recollection of its secondary position in the chain of causes is important.

Its function being to distribute the blood, the obvious desiderata are to give it no more than it ought to have, and that of the proper quality. Pure, so far as digestion is concerned; pure, from the effects of defective action in the liver and other organs, whose duty it may be to deprive the blood returning to the heart of any thing which it ought not to carry thither; pure, as to temperature, and any *other influence* which it may derive from its exposure to air in the lungs; and, lastly, that the heart be undisturbed, in that regular and efficient stroke by which it should distribute the blood, by mental inquietude. Now, it is difficult to perceive how bleeding, digitalis, and narcotics, can effect these objects. Bleeding may empty the vessels; but it will not prevent them being again filled, whilst it will often greatly increase the disturbance it is the object to allay; and as to the quality of the blood, that is not likely to be improved by digitalis, opium, or other narcotics. The only plan to keep the heart quiet, in my experience, is, to keep all the assimilating organs as tranquil as possible; to oppose any undue quantity of blood, by the substitution of a careful adjustment of that from

which it is originally made (the food); and, above all things, to keep the mind as cheerful and happy as possible. Disturbances of the heart are by no means uncommon occurrences in cases of tumour; and although I cannot quite agree with Sir Benjamin Brodie (if the journals report him correctly), that certain disturbances of the heart are not necessarily preventive of operations; yet I can say that they will generally yield to good treatment; and that in many cases, wherein there is little reason to doubt organic disease, the symptoms may be materially relieved, and life indefinitely prolonged.

The heart is another admirable example of the seat of disease and the seat of its real cause being seldom the same, and of the axiomatic character of the rule which seeks to adapt the agenda to the power.

Genital Organs.

I could relate a number of very curious cases illustrative of the influence that various conditions of the genital organs have had on local diseases; but I know not how to generalize them, so as to give them a practical bearing in the limits proposed to this volume. I shall conclude these hints by a very few remarks on the uterus. It has some important bearings on the system here recommended.

In some cases, it forms, if I may so express it, the unknown quantity in determining the actual condition of the system. I allude to those cases wherein, without, so far as you can learn, any symptom of disease having appeared at any time, it has

in married women failed to fulfil its functions. You never, I think, have this condition presented to you without detecting something wrong in the œconomy, besides the uterus. In some, the deranged actions of the system are palpable enough; but, in other cases, it is extremely difficult to refer the defect to any *definite* condition of any organ. It would be more discreet in me, perhaps, to say nothing further; but, as for many years I saw a great deal of uterine *disease*, and was obliged to investigate the matter, perhaps, rather more than is usually expected from consulting surgeons, I think it right to observe, that, both in regard to what I have already published, in connection with the sympathies of the uterus, or to some of the so-called *diseases* of pregnancy, and also from my experience up to the present time as to its actual disease, my opinion still remains the same —that the uterus is *generally* a secondarily affected organ; that the whole of the facts are reconcileable with such views; and that some of its more teazing as well as severe affections are seldom relieved permanently on any other.

We have often to deal with these organs in states of tumour; and I mention one form at once, because it is a sort of exception to some general rules of proceeding which I, in regard to operative proceedings, am about to recommend—I mean polypus. The *common* polypus of the uterus is not a disease necessarily connected with any serious, in the sense of *immediately* threatening, state of system; but it is often an affection accompanied by deficient power. The polypus may grow to an immense size without producing much, or in some cases any, inconvenience. More generally, however, after a time it begins to

produce irritation and sometimes hæmorrhage; thus I have known a woman nearly exhausted by repeated bleedings, and look as haggard and exangueous as if she had some highly malignant disease. I recollect a case of this kind in which I removed a polypus of unusual magnitude; but it was a common polypus, and the woman recovered after its removal, just as in an ordinary case. That all these diseases admit of cure, I feel pretty certain, from what I have seen of the effects of treatment on various affections of the mucous membranes of the nose, as polypus, and other more diffused alterations of the membrane. The uterus, however, presents *peculiar* sources of difficulty; and as to the site it so often offers for morbid deposition, it would seem that, if we may be allowed to use the word "*conservative*" in regard to malignant tumours, cancer is for such reason most frequently deposited on organs whose original functions may be said to have been fulfilled or not exercised; as when it occurs so commonly as it does in the breast or uterus of women who have not borne children, or who have passed the usual period of so doing. As it is one of those organs to which local measures may be applied, I shall add an observation or two on that subject in the next section. The regulation of the mind is, in *all diseases**, of the first importance. This fact is admitted, and its application in disease is progressing, but still too slowly. I have shewn, in two former works, how strongly impressed I am with the necessity of a more refined enquiry into the subject; and even pointed, as I think, at one mode in which

* It has been thought *specially* important in uterine affections. My experience affords me no proof of that.

it might be auspiciously cultivated. To repeat these observations here would be unnecessary; and to carry out that of which they form a sort of text, would take me far beyond the whole size of this volume; but that there are most valuable relations to be demonstrated, not only between the mind *generally* and the body, but *particular* disturbing tendencies between different mental emotions and *particular* organs, I think the facts at which I have glanced in the "Unity of the Body," and again in my "Medicine and Surgery," place beyond the possibility of dispute.

Of Measures applied locally to Tumours.

There is no such thing, in my opinion, as a "local application" in Surgery, if by that term is meant, a measure whose influence is necessarily confined to the part. Local measures may be very serviceable or very injurious, and both immediately or prospectively. A local measure may cause the cessation of a local action, and occasion its exhibition in some other site; which is doing apparent good, and real mischief; local measures may relieve the injurious tendencies of weight, unfavourable position, &c. They may, in certain cases, convert the actions of tumours into actions of surface; and, in some few cases, with advantage; they may exclude various injurious influences from the Tumours, as mechanical irritants, vicissitudes of temperature, &c.

They are evidently, then, conveniently divisible into such as profess to excite actions in the part, or such as are supposed to act by excluding such inju-

rious influences from without, as might impede or embarrass the salutary operations of nature, i. e. positive or negative. In the former, I have little confidence; the judicious management of the latter is very important in most cases of tumour.

Mechanical Irritation.—Under this head I include weight, depending position; friction of coarse, folded, or otherwise ill-adapted clothing; frequent examination, to which patients are naturally enough inclined; all which should be guarded against by support, careful adjustment of soft and smooth surfaces next the tumours, and impressive interdiction of all handling.

By *Chemical Irritation*, I mean unnecessary applications, ointments, lotions, &c. and changeable qualities of atmosphere, as variations of temperature, moisture, &c.

I am exceedingly distrustful of all medical "applications," which *generally seem* to do nothing, or *evidently* do mischief. That ointments of iodine and other things have been useful occasionally in promoting the absorption of deposition, is true; it is equally certain that they generally fail.

With regard to stimulating classes of application; generally, they appear, as Mr. Abernethy long ago used to say, to increase the actions going on in the part. If the absorbing action preponderates, they expedite the removal of the deposition; if the depositing action preponderates (chiefly the rule), they increase the tumour. Whilst I subscribe to the positions, I generally dispense with the remedy to which they refer. In the one case, they are clearly unnecessary; in the other, injurious.

Nature will remove the tumour as soon as its causes are removed. Any thing more than this,

except for reasons which I shall presently mention, may only replace the disease of tumour by some other.

That determination to the surface which is exemplified in the majority of diseases, and especially in the phenomena of skin affections, and of which tumour is a "falling short," may in some instances be advantageously accomplished by setons. In the Transactions of the Royal Medico-Chirurgical Society will be found a paper of mine, on their application to the more serious forms of *deep-seated* nævi, which, so far as I know, had not been successfully treated before. Setons had been often enough used in the common superficial nævus; a disease which, if left alone, will very often be removed without artificial assistance. I seldom, however, use setons in the treatment of tumours. Generally speaking, in these diseases, they are either dangerous or useless; and I should not myself employ them, except in the case I have mentioned; and, perhaps, in some special conditions of bronchocele, into which I cannot enter, as the discussion of particular tumours is not my present object.

The exclusion of injurious influences of atmosphere, &c. is occasionally happily managed by a bread and water poultice, *without grease;* and my experience enables me to appreciate all that Mr. Abernethy used to say as to it being properly made. I heard a gentleman once, in a lecture, generalize all sorts of poultices, by saying that on the whole they were to be regarded as warm and moist applications, and making very light of their influence on disease. Now, the real fact is, that different poultices are very different things. They are either very

beneficial or very mischievous; and, even though they contained nothing but warmth and moisture, I should think that a philosophical surgeon must be at little loss to see that they must be often very powerful agents.

A well-made bread and water poultice has these advantages. It is smooth and soft to the surface; it promotes the function of the skin covering the tumour (which is probably increased by the augmented actions going on beneath it). Increasing evaporation, it removes redundant heat (an universal stimulus to the action of deposition). It opposes the inconvenience of frequent manipulation on the part of the patient; and, by the comforting sensation usually imparted to the skin covering the tumour, it has a tendency, by means of the well-known sympathies of the whole skin with all its parts, to favor a tranquil condition of the whole organ. Now, a hard, thin, greasy poultice, or one not renewed with sufficient frequency, reverses all these conditions, and is so bad an application, that it is difficult to imagine any worse.

In practice, no one would imagine before hand the difficulty of obtaining the required conditions of a good poultice. I do not myself prescribe poultices as a general application in tumours, or unless there be some evidence of vehement action. I usually direct the skin of the part to be covered with smooth linen, or very fine flannel, according to the season; and then put on a bread and water poultice at night, if there be any heat, pain, or other evidence of activity. If the tumour be on the face, or other part *usually* exposed to the atmosphere, the avoidance of extreme temperature is all that is necessary:

the ordinary external relation of the skin in such parts renders almost any covering heating and pernicious.

I have little confidence in medicated poultices. I employed them extensively some years ago, and I will not say that they never do good; but I have much oftener seen them entirely fail, and excite hope only to disappoint it. I shall have to make a few observations on the active principle of many of such poultices in the concluding section. In certain stages of carcinoma of the uterus, I have occasionally heard of benefit having been derived from various medical measures: lotions of various salts of arsenic, zinc, lead, and narcotic principles, in weak solution. I have seen them repeatedly tried, without relief; and my experience, in this character of disease, leads me to regard extreme cleanliness, from use of tepid and sometimes cold water, with the careful defence of any accessible surface from acrid secretions by the interposition of spermaceti ointment, or any other mild oleaginous matter, as representing the principal benefit derivable from local measures. I have been very anxious to teach a strictly necessary and definite practice, which shall allow of reasoning which promptly tests its merits or its defects, and which constantly tends to the evolution of the principle on which the measure rests, either for good or harm. To all this, the expulsion of useless remedies, and the repression of that desire to be constantly doing something, greatly contributes. Measures of mere routine tend to persuade us that we are doing a great deal when we are doing nothing, or worse; fog our reasoning with unnecessary complications, and divert us from

concentrating whatever judgment or experience we may have in endeavouring to bring to light the true nature and relations of the disease. In short, what I would impress on the subject of local applications generally, as the result of my own experience, is that, as involving the removal of various disturbing influences, whether mechanical, chemical, or others, they are very material; but that, prescribed as agents for the production of new actions, they are measures on which I have very little reliance, in the first place; and I think that, in their most successful adoption, they are often objectionable; as replacing *effects* in other sites, instead of administering to *causes*.

I have seen a good deal of local remedies in other diseases; that is, where I have made ulcerated surfaces the portal for medicines, instead of imposing everything on the stomach; but this is a wide matter, of which I cannot here describe the bearings or results, and which is foreign to my present object.

Of the Removal of Tumours.

That a practice founded on the views of which I have endeavoured to give a sketch, and principles easily deducible from them, is capable of indefinitely diminishing the number of operations for tumour, my own experience has abundantly convinced me.

There are, however, many occasions on which the removal of a tumour becomes not only justifiable, but judicious. Firstly, we sometimes have

tumours presented to us, when, either mechanically, or in communicating irritation, they are disturbing the functions of parts essential to life; as when they affect the trachea. I have known a *small* tumour, placed in this situation, accompanied with intolerable irritation and paroxysms of dyspnœa; and another case, one of nearly a foot in diameter, produce no irritation at all. Secondly, we are consulted when they have already become so contiguous to large blood-vessels, or other important parts, as to suggest that, should circumstances require removal, the danger may be materially increased by any extension of their present connections. I have removed tumours from the neck and upper part of the thigh in this way, when the immediate cause of the operation was the proximity of a tumour to important parts, with which it was as yet unconnected.

Tumours also occur about the face and side of the jaw, the removal of which sometimes involves injury to the facial nerve or the parotid duct; and I have sometimes removed a tumour, perhaps when I should not otherwise have done so, because it was evident that any extension of it would probably connect it with the parts I have mentioned.

In such cases, then, though you gained only an alteration of site by your operation, it would be acceptable; but the return of the disease in any form is not necessary; for although you lose the beautiful index of the progress of your treatment, afforded by the tumour, still your enquiry into its causes, and your means for their correction, may be just as rigid as if the tumour remained.

In some cases, the mere presence of the tumour

keeps your patient's mind in a state of continual apprehension of cancer; and if you cannot allay that, you will strive in vain, with so seriously a disturbing interference, to correct the various functions. In general, it is true that, if a patient is not tampered with, you may calm this inquietude; but if you cannot by other means, the removal of the tumour is justifiable; and the measures which you have instituted unsuccessfully to check its growth or promote its absorption will (with the interference of mental inquietude removed) probably prove efficacious in preventing its return; to which, so far as the assistance of the patient goes, the operation will contribute.

In fact, in some cases nothing will induce patients to be steady to a course which they have found beneficial. A tumour has been painful and enlarging. It has become quiet and stationary; even diminution has commenced, and then the patient begins to take liberties; and this happens again and again. In such a case, the removal of a tumour has often been followed by persistent caution and by permanently successful results.

Not long ago, I was obliged to remove a tumour under the following circumstances. A female from the country consulted me for a firm tumour on the side of the face, which lay on the masseter muscle. It had existed for some years; and it was now represented as becoming painful and increasing in size. Under treatment, the tumour soon became stationary; and I have very little doubt but that its absorption might have been procured. The patient, however, after a little while, came to me repeatedly in a state of great alarm. She said that a medical

gentleman, in her neighbourhood, who had seen her previously to her consulting me, was constantly calling on her and begging her to have it removed, for that it was endangering her life to retain it. I allowed her to make two or three visits before I made any other remark than to reassure her; but finding that she was still molested, I said at once that I could do nothing more, with her mind in that state of excitement; and, therefore, I thought she had better get rid of it, and be at ease. She perfectly coincided in my view, and I removed it. It was a very hard, firm tumour, but certainly not carcinoma. It was very close to the parotid duct, which I easily avoided, in making, at the suggestion of Mr. Phillips of Islington, who assisted me on the occasion, a semicircular incision, and laying down the flap, which gave the required room for its removal, without wounding any of the parts I wished to avoid.

Common polypi are a curious sort of tumour, and one of those which require removal. They sometimes grow, from about the neck of the uterus, to a very large size, and often by a very narrow pedicle.

The discovery of their existence has been often accidental; but, as I have observed already, after a time they begin to produce irritation, &c. Polypi of the nose also require removal, from the mechanical impediment they form to respiration. Their return and their progress may be controlled by measures directed to the general health, which I have often seen; but I have not had an opportunity of trying this in uterine polypi, though I have removed many, and, so far as I know, without return.

Another condition of things under which an operation for removal appears to me desirable, sometimes, is the following.

The tumour is large and increasing; you institute your treatment, you succeed in stopping its growth; but the absorption of so large a mass may be doubtful—may imply the necessity of a long continuance of vigorous attention on the part of your patient, which you may not be able to command; or, in certain states of constitution, it may involve an effort which it may be a sound œconomy of power to spare.

Chimney sweep's cancer is a peculiar case; and, in the *present state of our knowledge* on the subject, I should think it most safe to adopt the ordinary treatment, and remove it.

This disease requires investigation. Dupuytren shewed a case of it in Paris, where they have little coal smoke, as rare; here it is very common. I have removed a great many of them. I shall briefly allude to a case cured without operation; and I have heard of one or two others in the hands of other surgeons.

Apart from the additional circumstances of connection with important facts, or embarrassment of respiration, or other function, nothing appears to me to be an evidence of more defective reasoning than determining the excision of a tumour by the *fact of its getting larger.* The increase of a tumour is a plain proof that its causes are still in operation; and we cannot be surprised that the excision of tumours on *such* grounds should be followed by their return; for, in the first place, I have very rarely indeed seen any directions, worthy of being so called,

given to a patient after the operation, with a view to prevent the return; and almost never any investigation which could with reasonable certainty place such directions on a clear and intelligible basis. Again, as I have observed, the index presented by the tumour, by which we may most satisfactorily test the accuracy of our views, is gone, where the certainty of the persistence of the cause would most require it. In the removal of tumours by operation, another thing not wholly overlooked, but marvellously little attended to, is that the absence of return of the *tumours* is no proof that the operation has had any effect in arresting the progress and preventing the recurrence of *disease*. When I say marvellously *overlooked*, I mean that the language held is frequently this—"The tumour removed never returned, the patient died so and so, but of a different disease," &c. Now, I do not mean to say that, when a patient has died, some two or three years or more after the removal of a tumour, it is right to assume that he died of *the* same disease in some other site; but I do mean to say, that, in the absence of accidental causes or other precise information, it is a pure assumption to conclude the contrary: neither is it consistent with our own admitted reasoning in other matters, with the true relations of which their very frequency obliges us to be familiar, but of which we are not apt to extend the application. Ulcers in the extremities are not unfrequently healed, and followed by apoplexy, inflammatory affections of the chest, &c.; now here, notwithstanding the differences of appearance, we recognize these occurrences as composing one chain of morbid phenomena; but we do not expect to find an ulcer in

the brain, nor in the chest, much less one presenting characters similar to those in the extremities. A recognition of such relations seems seldom applied to tumours, unless the physical appearance of the disease should be that of tumour, which is clearly not a necessary circumstance. The fact is, that tumours are mere *symptoms* of deranged organs; they lie intermediately between affections of the surface of the body and the viscera; and if a tumour be removed, or an ulcer, without attending to the cause of its production, it must be followed, sooner or later, by some internal disease, or another tumour. The general relation of the morbid phenomena to disordered conditions of the system is not *necessarily* affected by the *site*, still less by physical appearance. That particular relations are every day being exemplified by these differences, and that perseverance and true modes of study will in time render them beautifully instructive, I have not the smallest doubt.

Even the "Law of Inflammation" obviously affords a general relation of *power* to the *site* of a diseased action; and that is to me a considerable guide, by which I trust that sites of diseases may not only in time be the better understood, but also why, in those myriads of diseases seen on the skin, we have papulæ taking place in one case, vesicles in another, pustules in a third, and so on.

As I am not treating of the nature and treatment of *particular* tumours, I cannot enter into the subject of the mode of conducting the various operations for their removal.

In general, they are sufficiently simple; and although they, of course, involve many differences in different kinds of tumour, and in different situa-

tions, yet they are all plainly deducible from the nature of the case, and are usually treated of in elementary treatises on surgery.

The principles on which I proceed in preparing a patient for operation will be understood from the general contents of this volume.

M. Le Roy d'Etiolles has collected some statistics of cancer throughout France, which emphatically, so far as they go, impress misgivings as to removal by operation; inasmuch as out of 1172 patients not operated on, eighteen lived for more than thirty years after the first appearance of disease; whilst in 801 operated on by excision or caustic, four only lived to that period.

Certain it is that one interference or another places us in a position in which we hardly know what *is* the *natural* termination of cancer; and in my own experience, every case that has been one of intense suffering has not only been subjected to very obviously injurious influences, but almost always to treatment demonstrably injudicious. All this impressively urges the necessity of more enlarged investigation.

CAP. VI.

OF THE APPLICATION OF THE FOREGOING PRINCIPLES OF PRACTICE TO THE TREATMENT OF TUMOURS. THE GENERAL CHARACTER OF THE RESULTS.—CONCLUSION.

If the principles and practice, recommended in the preceding pages, appear as simple as they really are when properly understood, nothing is more easy than to bring whatever claim they may have to our confidence to the test of practical application. Nothing more is required than common sense and average information, combined with industry; and if a little more of the latter be necessary than is usually employed in medical investigations, this ought to be no impediment; for industry, though a homely quality, is equally inseparable from all other enquiries into the laws of nature, and one equally within the grasp of every order of intellect.

That the tendency of the plan as a whole is to improve the general condition of the body, is obvious

and indisputable; and, therefore, if it procure not the absorption of a tumour, it can by no *perceptible* possibility do mischief. It proposes no new system; it rests on no hypothesis; it takes the facts as they exist already, and enjoins only the same kind of use of them as that *already made* in most other branches of science. The primary requisition it establishes is simply a more enlarged enquiry into *all* the facts, and a *synchronous* examination of their bearings on *all* the functions.

If such be the character of the plan, it would in common fairness seem to deserve a trial. In my gradual development of it, in relation to the treatment of tumours, however imperfect it may still be, I could say a great deal of the labour it has imposed, and how many beautiful illustrations have been spoilt in dispensary practice, after a long and toilsome conduct of a case to the very goal of recovery, when, all apprehension of danger having subsided, the patient has relinquished attendance—cases which, however instructive, were thus rendered useless for future demonstration. Many things, however, which involve labour and perplexity in development, become very simple and easy in application; and this is no bad test of excellence.

The results are the means by which any plan of practice must be tried. "Organic surgery," as I have called it, in my experience, has appeared to obtain results which I do not believe to be *as yet* attainable by any other method. In resting the cure of disease on a more searching enquiry and constant endeavour to remove interferences which impede the operations of nature, it tends to unfold

and *define* much more clearly the causes of disease. It thus contributes to the advancement of science, and places recoveries on a much more permanent basis, by putting the patient in possession of the facts on which the recurrence of disease must depend. A patient could not be always taking medicine, for example, with advantage, or even with impunity; but he might be always avoiding influences, whether of habit, diet, or whatever kind, which in his own experience had been proved to be specially disturbing to him.

Further, the most successful application of organic surgery is accompanied by so stringent a measurement of our actual knowledge, as to impress nothing more strongly than its diminutive extent, as compared with that which is unknown. It thus represses vanity, whilst it excites our thirst for improvement; and at the same time points, through a perspective, as I think beautifully lucid, to the sources, and even the paths leading to them, whence alone we can hope to obtain it.

In developing the "Law of Inflammation,*" I have shewn how, in many cases, the most severe inflammations may, in the exposition of their *real* causes, be quickly and successfully treated without the loss of a drop of blood. I have also frequently shewn how "organic surgery" deals with cutaneous diseases, and that it is never more triumphant than in those affections which are ordinarily most obstinate.

I have also exemplified how readily, in diseases of the rectum, it prevents the necessity of opera-

* "Medicine and Surgery."

tions, or reduces those of severity or magnitude to such as involve scarcely any risk, and indefinitely diminished suffering.

With respect to tumours, I shall best convey my own impressions by showing, as quickly and briefly as I can, the general order of the occurrences; whence they have arisen; and how it happens that they were first awakened, as it were, and have since been so much deepened by the consideration of malignant tumours.

I will do this as connectedly as I can; but as I have to select it from somewhat bulky manuscripts, I scarcely know how to arrange the matter, unless I had more space, or were treating of individual varieties of tumours, which is not my present object.

About twenty-five years ago, I became professionally connected with institutions which brought me largely in contact with the masses of people who inhabited some of the most densely populated districts of London; and the absolute number submitted to my inspection was greatly increased by one of these institutions (the Finsbury Dispensary), embracing a very extensive district in London, and having only *one* surgeon. It was the rule of the Dispensary to allow the patients to apply to the Surgeon, under certain circumstances, before nine in the morning; and this, with the permission to come the first time without letters, occasioned a daily crowd of applicants, which, on my predecessor's death, were transferred to my house, so far as the institution went in which I succeeded him.

I had thus not only abundant opportunity, but every facility, of classifying cases for my own im-

provement and for my clinical lectures*, as well as the more efficient working of the whole. Amongst the various operations I was called on to perform, many were for the removal of tumours; and many of them, of course, were of cancerous or otherwise malignant character.

As I endeavoured, both by precept and example, to enforce a practice founded on the principles I had imbibed from Mr. Abernethy, I always tried to get my patient into the best possible condition before I performed the operation; not by bleeding or any special proceeding in every case, but by endeavouring, in all, to get the various functions into the best condition I could; and then the tumour was extirpated.

Although it would often happen that the section of a tumour would present appearances very different from those expected, and a benign disease offer those indicative of malignity, I cannot recollect any instance in which I removed a tumour, believing it to be malignant, where the physical character contradicted the diagnosis. With the general impression of surgeons as to these I was familiar, from what I saw, at times, for so many years at St. Bartholomew's, and many other hospitals. As my opportunities were as extensive as I desired, it was natural that I should improve in my management of the different organs, become acquainted with the injurious nature of peculiar localities, particular employments, &c. Now, I *believe* that I had only a just confidence (for I had

* Clinical lectures are now become general, as they ought to have been long before. The lectures alluded to were the first given in London.

repeatedly tried it by the only tests) in myself, when I could distinguish malignant disease with considerable certainty. Well, as I proceeded, I found the following circumstance happen. A patient applied with a tumour, which, having been examined with care as to its nature, site, age, and other circumstances, was pronounced malignant, and recommended for operation. In the mean time, such measures for the improvement of health were instituted as the case appeared strictly to require. When examining the tumour, for the last time previous to the operation, some change would be perceptible. This would, of course, under the conventional impression, lead to a doubt of its malignancy. The operation would be deferred, and the tumour would become absorbed. Now, I saw this occurrence several times—notwithstanding I had no previous reason to doubt my diagnosis—before I ever thought of forming any conclusion or making any other statement to the pupils than that I had mistaken the case. Still I began to examine with more industry than ever; and at last, after having pronounced a tumour malignant, I used to add, that nevertheless I had seen such tumours, as I thought, become absorbed under improved health; and the tumour in the case in question became an instance of it. I recollect two cases occurring about the same time. In one of them the operation was not thought of, because the woman had symptoms of diseased heart. The tumour had become less, not being wholly absorbed when I lost sight of her. Still, so far as the principle involved went, I was nearly as much excited by seeing a tumour, which I determined on as malignant, diminish, as

if the whole had been absorbed; because it appeared to me that the real difficulty, like the present dogma, lay in the premier pas. I now began really to try to influence the absorption of malignant tumours; and I believe that I have done it in several instances. I should say as many as ten in twelve, at least. This may seem a very small number, perhaps, when mistakes are allowed for, &c. Granted; but let the whole truth be told. Many of those which did not terminate successfully were, to my mind, not a whit less instructive. It is the most difficult thing in the world to get patients to be permanently obedient so soon as pain is gone; and the tranquillity of the tumour seems indefinitely to postpone danger. Some little liberties are taken. In malignant disease, your plan once established, I would beg most emphatically to insist that no trifling whatever is admissible. If I were asked from what description of case I have most permanently sealed my conviction, that, with further investigation, cancer will become (as long as the viscera are sound) a curable disease, as I devoutly believe it to be in many instances already; I should say, from cases of the following kind, where, notwithstanding the result, the evidences of the nature of the disease and of the influence of treatment are alike beyond the power of dispute.

A lady, ætat. 39, consulted me for a tumour of breast under circumstances that were as hopeless as it is possible to imagine; and it is a very good case, so far as shewing what may be done where there is organic disease.

She had had the tumour for some years; and I found, in my notes of the first conversation with me,

that she had consulted four surgeons, three of them men of great eminence in London; besides some physicians; and that they had—some more, some less strongly—urged its removal. To this she would not consent.

On examining her breast, I found that which was described in my notes at the time as a "true specimen of carcinoma, very hard, and adherent to the subjacent parts. The skin is tucked in at the nipple, and a dark spot there, slightly abraded. It is painful, with a "sense of drawing." Arm swollen, and red in the vicinity of the tumour. There issues from the dark spot a few drops of blood daily, which began when she made use of, as she believes, some irritating application.

In this lady, I could not find any one function in her whole body going rightly, except the kidney, and that was only from her own report; and yet I could find nothing unusual in her habits, except that from about the age of 20, previously to which she had been very active, her habits had become sedentary, catamenia were scanty and pale, and never lasting more than a day; liver extremely torpid, skin chilly, bowels habitually costive, appetite deficient. She had a number of other symptoms, such as frequent palpitations; but, as she called on me, I did not examine the condition of the abdomen until I visited her, when I found the liver hard and enlarged. Her tongue was very peculiar; I never remember to have seen any like it in a living person; it was shrivelled and pale, covered with a remarkably thick coating of something that I never saw before, but not unlike the tongue that you sometimes see after death in advanced stages of putrefac-

tion. She knew perfectly well that there was no chance of cure; but as she suffered greatly, and seemed very willing to do any thing that was recommended, I did every thing I could to persuade her to be careful; assuring her that, although we did not allow ourselves to talk of curing such diseases as that she laboured under, yet, they were far from being beyond the control of treatment altogether; that a careful mode of life, adapted to the peculiar fault in the organs, was often rewarded by an almost entire immunity from pain, and a stationary condition of the tumour; and that I should not despair of accomplishing something in her case; not, said I, so much from any *peculiarity* of plan, as that I cannot glean from you that any very particular attention has at any time been paid to your diet and mode of life, by which alone your organs can be got into better condition. Well, we set to work, and before she had been three weeks on the plan, she said that the tumour had not been so easy for twelve preceding months. For the treatment, so far as I directed it, consisted of very plain diet, the rigid exclusion of sugar and grease of all kinds—friction to the skin—with especial avoidance generally of any in the neighbourhood of the tumour; when the catamenia occurred, such as they were—a few leeches were ordered to be applied to the pubes on their cessation. Her medicines were chiefly aloes and ipecacuanha, with now and then three grains of calomel, so guarded as not to act too quickly; but aloes and ipecacuanha were the medicines generally used. Now this case went on until the tumour had become decidedly loose, that is, more moveable, and the suffering from it very trivial; it

was now and then a little uncomfortable, but she was generally *easy*. Although she herself referred her uneasy moments to slight aberrations from the plan, still she could not help occasionally transgressing; and an unfortunate consultation served only to render matters worse, in the following manner.

Finding her thus getting irregular, I availed myself of an accidental mention of the name of an eminent physician whom she knew, to propose that he should see her, to which she willingly consented, and we accordingly met. We did not retire; but he said, very frankly, that she must live quietly, but, for his part, he thought she might eat what she pleased. What do you say to that, Mr. Macilwain? said the patient. I rejoined, well, I am glad to hear it, because you will now give me an opportunity of seeing whether my directions have been generally right, unnecessarily stringent, and so forth. She began, therefore, to live like the rest of the establishment, not doing anything that was very extraordinary, but altogether neglecting that peculiarity in diet, which my investigation of her case had led me to prescribe. The result was, that in a few days she was thrown into a state of most terrible suffering; for this, opium* was prescribed, without any relief; but the return to the former plan was attended with almost immediate amelioration. She never, however, recovered her former ease. The only thing that gave her relief was a very strict diet, and occasionally any thing that afforded evidence of biliary secretion. The pain eventually seemed to leave the breast at times, and to have merged in a kind of rheumatic pain in the joints, with accumulated sufferings and an almost total loss of appetite and capability to retain

* She had taken no narcotic previously to this.

food. This poor lady, I believe, hastened her death by her own imprudence; because her case could not be more desperate,—none more entirely precluding hope of any thing like cure; yet, as long as she adhered strictly to the plan, she had almost absolute immunity from pain; she experienced this too often not to allow it. She frequently tampered, with impunity; but once too often: and the result was what I have stated—she died in great agony.

Now this is the class of case that I regard as the most instructive; because the demonstration here is complete: hence there is no saying that you have mistaken the case, sir; for the disease has killed the patient: that is a pretty good proof of malignancy. You assert the tumour is malignant, you get rid of the symptoms, you alter the character of the tumour. Now the question is, why? you allege that it is the avoidance of this measure,—the adoption of that,—in short, the treatment? But how are you to know—can you advise your patient to do what is wrong, to try the question? No; but this most valuable question of nature, the imprudence of your patients, asks for you, and the symptoms and tumour return. What do you wish more? why to know, say you, that the tumour is malignant. Well, your patient ventures to trifle once too often, and death gives you the proof you desire.

I have no room for multiplying illustrations of particular points. I could mention a very remarkable case, which in its commencement presented a tumour of extreme hardness; and in which ultimately the characters of cancer and fungus hæmatodes were combined. This woman, under every possible disadvantage, not only obtained consider-

able advantage at the beginning, but those changes actually had begun, which I knew to be indicative of further benefit, when she became careless. Nothing could induce her to avoid trifling with her case; and she died, at the same time a true specimen of the diseases which I have mentioned, and proof of how much they may be controlled by appropriate modes of life, &c. &c.

Some patients, warned in time, retain the benefit they have gained. Take a case. I shall avoid every particularity in its description, because I have not the slightest objection to any one doubting its nature. I would rather lead them to work at the matter themselves; that is the only way to educe similar results, and, what will alone insure continued investigation, similar convictions.

A lady, about forty-five years of age, applied to me for advice for a tumour of the breast, which had all the history and characters of true carcinoma.

Having examined it, and found that its connections were loose, and that it otherwise presented those conditions which are ordinarily recognized as suggesting removal, I told her that I could not conceal from her that it was a tumour of very serious character; and that, in short, when her health was got into a proper state, in all probability I should advise its removal. I then said, as is my custom, "Now do you think that you can implicitly do every thing that I tell you? because if you will, I think I can give you good reasons to encourage you; if otherwise, you had better consult some other surgeon; for I never profess to do any good without the co-operation of the patient." On her assuring me that she would be obedient, I said, "Then I

think I shall be able to put you in a condition in which the removal of the tumour (should that be the course proposed) will be effected under the most auspicious circumstances; perhaps I may place you in a condition in which the removal may be a matter of choice; and, lastly, although we do not," said I, "allow ourselves to talk of curing tumours of this description, yet, in some very few instances, where patients have been particularly observant of the rules, such tumours have entirely disappeared. Their disappearance, of course, forbids my positively asserting that they were tumours of this description; nevertheless (although such disappearance is in the last degree improbable), I mention this fact to you as an encouragement to persevere in any plan of life I may propose."

In a few weeks, certain changes, hereafter to be mentioned, occurred in the tumour; and in a few months it entirely disappeared. On this, I told her I could not too strongly recommend her to continue her present mode of life, which, in fact, involved no privation, its leading feature being regular and daily exercise, a plain diet, the exclusion of *saccharine* and *oleaginous* matters, and alcohol.

About a year after this, she again visited me, with a small deposition in the same breast, accompanied by some uneasiness, though scarcely amounting to pain. It seemed that she had resumed her former habits, which involved no intemperance; but still the ordinary diet of other people, including the articles she had been forbidden; and she also said that circumstances had interfered with the regularity of her exercise. The former treatment was now resumed, and with similar success.

The following case of malignant tumour, as well as that, in a subsequent page, of thickened tunica vaginalis, were published in the Lancet, June, 1842-3; but as I can select no better to prove their respective points, I reprint them. The first case was sent me by Mr. Kingdon. My former associations with this gentleman, whilst they constitute, both professionally and socially, one of my most pleasing retrospections, also enable me to bear witness to his experience in malignant tumours.

A lady, about thirty-four years of age, had a very hard tumour in the centre and occupying about one-third of the upper lip. It had a very firm and well-defined boundary, was of a circular form, and, though deeply imbedded in the substance of the part, was very moveable. The lower surface of the tumour was denuded of its integuments, presenting an excoriated rather than an ulcerated surface. She suffered considerable pain occasionally, and the denuded surface was exquisitely sensitive. Some months had elapsed since her attention had been first excited to it, and the tumour had gradually acquired its present characters. She appeared much out of health; a bilious, dull, leaden complexion was accompanied by deficient appetite, irregular and painful menstruation, torpid bowels, cold skin, pain in the head, &c. Having carefully examined the case, I told her that I feared nothing could be of any service but the removal of the disease, and that I perfectly concurred in the advice given by Mr. Kingdon, viz. that so soon as her health was somewhat improved to allow him to remove it. Mr. Kingdon wished me to take an analytical account of her case, and to try whether it

were possible, by any measure, to influence the condition of the tumour. I, therefore, took down her case in the tabular form which I recommend; and, on a careful review of the history and present phenomena, was led to regard the liver and uterus as the organs *primarily* and chiefly affected. Not to enter unnecessarily into details, I may briefly state that the organs to which my endeavours were directed were the liver, skin, and uterus. Her diet was simple and strictly defined, and she kept memoranda of the various articles of diet she employed, together with such other matters as I recommended, after the plan of which I have already spoken. See p. 99. The lip was to be kept still; she was to speak as little as possible, and to take her food through the spout of a tea-pot. She was allowed to put a bread and water poultice to the lip at night, *when the tumour was painful*, and to defend it from the atmosphere in the day-time by a little spermaceti ointment, applied warm, by means of a camel-hair brush. The medicines she took were aloes, antimony, or ipecacuanha, and confection of opium, in different modifications and doses, according to her condition, and now and then, but rarely, a single dose of calomel and confection of opium. Besides these, until the skin became more tractable (as I wished the kidney to be more liberal), I gave her nitrate of potash with sarsaparilla. She was to take daily exercise, and to have her skin well rubbed. She, also, in the course of the treatment, had a tartar-emetic plaster applied to the pubes, which appeared an useful auxiliary in restoring a more healthy condition of the catamenia. The whole treatment lasted six months. At first, her looks

began to improve, then her functions to become more regular, and at length alterations were observed in the tumour, first a softening, and subsequently a diminution of its bulk. The absorption continued slowly but progressively, until the whole tumour had disappeared.

Mr. Kingdon's authority is too valuable to be omitted; I, therefore, add his letter on the occasion:—

"New Bank-buildings, May 18.

"My dear Macilwain,—The young woman to whom you allude was brought to me with a sort of nodule on the upper lip, which in my opinion displayed the character of a malignant tumour, and not only from its own appearance and consistence, but from the general appearance and character of the patient herself, which was such as would lead any experienced surgeon to apprehend some malignant disease, had there been none manifest. The tumour occupied one-third of the upper lip at its centre, or, at all events, would have required more than one-third of the lip to remove the disease, which I thought would be necessary. With this unfavourable impression of the case, I availed myself of your ready assistance, and sent her to you, desiring her to pursue any plan you might direct, and come to me again in a week. When next she came, I saw no difference, and I acknowledge smiled at her description of your mode of investigation; but it had obtained her confidence; and, as the tumour had by no means increased either in size or malignity of appearance, I desired her to continue under your care as long as you would have the goodness to attend to her, and to see me occasionally. There

was soon a manifest improvement in her general health. Her face gradually lost its cadaverous appearance; her movements became more active, and her carriage more easy; but it was long before any material diminution was evident in the tumour, though its character from time to time was materially altered, until the whole of its apparent malignity disappeared, and then it became gradually absorbed, and disappeared also. I kept no notes of the case, as I knew that you did, and preferred to be a mere looker-on during your treatment; so that during the few weeks you were in the country I acted only as your assistant, modifying your prescriptions and your directions in accordance with your previous arrangements, as conveyed to me by memoranda put into my hands at the time. The case is wholly yours. I am only a looker-on, for I did not anticipate any relief but by excision.

"I am now convinced that many diseases which we have hitherto considered incurable, may be cured by that close attention to the minutiæ of function which *does not permit the most insignificant portion of the frame to be overlooked;* but the labour, the patience, the tact, required for the investigation, and all, necessarily, reiterated at each visit, with reference to previous notes, will most assuredly delay the adoption of the plan for many years, if not prohibit it altogether.

"I most sincerely rejoice, and at the same time wonder, at your successful treatment of this case. I remain, my dear Macilwain, as ever, most sincerely yours,

"W. KINGDON.

"To George Macilwain, Esq."

I could multiply these cases by the narrative of others; but I am writing on tumours generally, and I have no wish to hurry conviction. My earnest desire is to induce people *to think*, and to give to *all tumours* a more careful investigation. So far as the *powers* of the œconomy are concerned, I have *no* doubt that cancer is a curable disease; that is, so long as the viscera are sound; and that they are sound in many cases I feel certain. Moreover, I believe that, at the commencement, the deposition takes place in obedience to the law I enunciated in "Medicine and Surgery," &c.

Some years ago, a woman applied to me with tumour in the breast, having, as I thought, the characters of fungus hæmatodes intermixed with portions of carcinomatous deposition.

The tumour was very loosely connected with the subjacent parts; and, as nothing appeared wrong, so far as symptoms went, but which might be easily corrected, I told her that I feared the tumour would require removal, and, indeed, recommended that course, so soon as her health could be got into good condition. She, however, said she could not submit; and I saw nothing more of her for about a year and a half, as nearly as I can recollect. She then visited me; and, having described her sufferings, requested me to remove the tumour. I found that it had become much enlarged; that it had burst, and bled, and assumed all the characters of fungus hæmatodes. It had become adherent. The glands in the axilla were affected; and, in fact, all things had gone too far to justify any idea of operation. Well, this patient died. I had an opportunity of examining the body; and the disease was a

true specimen of that which I had first suspected it; *but the viscera were sound.* This was many years ago, before I had begun to think of these affections as subsequent cases have now obliged me to do.

I know a lady, at this time, who, as long as thirty years ago, was a patient of Mr. Abernethy's. She had a tumour in the breast, which was considered cancerous; and Mr. Cline and Mr. Heaviside had recommended its removal. On a journey to London, she called on my father, who was induced (I believe from some doubt as to the soundness of the opposite gland) to advise her to shew it to Mr. Abernethy before she submitted to the operation. She did so. Mr. Abernethy would not recommend its removal; but gave her a lecture on diet, and the mode of life which she should pursue. She was a very obedient patient; and the result was that the tumour became stationary, and ceased to be troublesome. No operation was performed, and she is now living in as good health as most people of her age, which is seventy. I have seen her very frequently since Mr. Abernethy's death; and she has occasionally consulted me for trivial ailments, but never on account of the tumour. Last year, she was attacked, in the country, with pleuritis, as reported; and for some time was in a very precarious condition; she, however, perfectly recovered.

I lately heard that there was now no appearance of tumour; but as she resides a long distance from London, I have no opportunity of verifying this statement from my own observation.

Many years ago, I recollect a patient of Mr.

Abernethy's, in St. Bartholomew's Hospital, who had a malignant disease of the lip, of that form which somewhat resembles a cauliflower, and who perfectly recovered. Not the least interesting point in the history, given by the patient, was that his father had died of a similar disease.

The malignant disease which affects chimney sweepers in this country, although connected with their occupation, is evidently dependent on some individual peculiarity of condition; because, out of the hundreds following that occupation, comparatively few are affected. I have seen one very bad case recover. A boy about twelve years of age was admitted into the dispensary, with a malignant-looking disease, occupying a considerable portion of the nates and the immediate vicinity of the anus. He was brought remarkably clean, so that I observed to the pupils, "That any body seeing that disease in a chimney sweeper, would immediately say that it was chimney sweep's cancer." "Sir," said the mother, "he is a chimney sweep;" as in fact he was; but, seeing the perfect cleanliness of his person, I had not thought of his occupation, which was only elicited by the remark suggested by the characters of the disease. This boy, by measures directed to his general health, abstinence from his employment, and perfect cleanliness, completely recovered.

I had drawn out short statements of the state of different functions, and the injurious influences to which patients had been subjected, selected from such cases of ordinary cancer as seemed to combine them in greatest number; but I am haunted by the fear of making a large book, and by the desire of

avoiding the discussion of particular tumours, which must be a separate task. I will, therefore, close these observations on cancer as quickly as I can, by stating the changes which those tumours, which *appeared to have* the characters of true carcinoma, exhibited, in their progress to absorption, of which I have seen not less than ten or twelve examples.

The first change observed is that, if there have been much pain, there is a very material and marked diminution, or a total subsidence of it, and without the influence of *opium or any other narcotic;* and this, too, when opium and other narcotics have been exhibited in vain. The tumour becomes loosened as to its subjacent connections; and I have more than once seen a tucked-in nipple resume the natural appearance. A change yet more important is, that the tumour which had before, perhaps, presented itself in one mass, with more or less irregularity, becomes broken into portions, so as to feel like separate depositions intersected, as it were, by lines of more healthy structure. This is speedily followed by a diminution of the characteristic hardness; so that, after a while, the carcinomatous character becomes entirely lost. The change progressing, the tumour becomes gradually absorbed, until nothing remains but what, if now examined for the first time, might be taken for an enlarged gland; and, this gradually disappearing, the part resumes its ordinary character. Now, all this has never happened without a change in some one or more important function to which the treatment has been specially directed, though this has varied in different cases according to the function which, on careful

analysis, appeared to be most seriously or primarily affected. If I were called on to name any dietetic measure most general, I should say the diminution of carbon, in the interdiction of grease, sugar, and alcohol. If I were asked what one organ has most frequently appeared to take the lead, I should say the liver.

With regard to all the diseases not yet brought within our power, I would beg to observe that no malady can be cured by those who are determined to regard it as incurable. Men will never *effectually* strive to do things which they despair of being able to accomplish.

Diseases *so* regarded *must* be left to quacks and quackery: that is, to men who will promise any thing for money; or to modes of random firing at the disordered conditions of the system, by (so-called) specific or other remedies; which (though of a different kind truly) are not, in my view, less quackery for being instituted by the regular surgeon.

Experience has demonstrated that the most simple truths have been too often neglected or forgotten; and that one of these is, that the state of our knowledge is, of all criteria, the most fallacious estimate possible of the powers of the animal œconomy.

There was a time when every aneurism involved a more severe operation than any so-called incurable tumour; for the limb was either amputated, or a yet more severe and dangerous operation instituted; too often both; when every unfortunate wretch that had a sinus, had it divided; and when the dressing of most wounds represented little more

than a series of interferences with the operations of nature. All this barber-surgery is now replaced by more correct views of the nature and relation of diseased to healthy processes; and much more is in course of departure.

In our own times, however, we have had, as it appears to me, a very humiliating illustration of how necessary that caution is which distrusts conclusions as to the powers of the body, founded merely on our existing knowledge or experience.

For thirty years, fractured neck of the thigh-bone, within the capsule, was, to a vast number of patients who sustained that accident, incurable; and if we enquire the reason, we shall be obliged, I think, to answer, "because Sir Astley Cooper said and taught so; and adopted a treatment which too surely verified the opinion."

The influence of this would have been more extensively pernicious, had not Mr. Abernethy, fortunately to a large class also, taught the opposite doctrine. I believe I had verified it in my own practice twenty years ago; and I found some of the first surgeons in Paris concurring with Mr. Abernethy. Still, as people got well, there was no opportunity of producing for demonstration such a fracture united, until, comparatively speaking, very recently; and now, a true view is taken not only of the nature of the accident, but of the difficulties by which it is attended; and these are met by the appropriate treatment which they clearly enough suggest.

Cancer must necessarily be a difficult disease in all cases; and for a long time, probably, in many incurable. It is a declaration of the impression of

long-standing and injurious influences. It occurs at a time of life when a thousand causes (time inclusive) have tended to diminish the sum of vital power; and too often to harass that which remains by numerous and complicated impediments. Therefore, in the nature of things, it must require the most searching investigation,—one that takes nothing for granted without evidence; and the whole proceeding conducted on those principles of inductive enquiry (until we know better) to which we owe *every* advance in *every* science. I have no wish to underrate the difficulty; but I would submit that in many cases it is far from insuperable; whilst in some the arrest of deposition is comparatively easy. However determined and zealous a medical man may be, it must be admitted that he will be exceedingly discouraged by the irresolution and defective obedience of the patient; and his efforts not unfrequently paralyzed as it were, on perceiving, whenever there is any real amendment, what trivial indulgences patients will place in competition with that perseverance, alike suggested by a just estimate of his labour and anxiety, and the terrific suffering which, sooner or later, is too generally the accompaniment of this formidable disease.

Still, if he will persevere, he will meet now and then with a patient whose firmness will cheer him on in his search for truth; and in whom, if I mistake not, success will reward his efforts. In the mean time, so far from wishing any one to take my statements, without examination for himself, I would say, that if they have any use, it will be in exciting

in others the same kind of investigation, whenever the opportunity occurs.

With regard to other depositions, I can only say that I very seldom see any now which I cannot influence by strict treatment: sometimes, in procuring their absorption; almost always in arresting their progress. I could exemplify each of these positions by the relation of many cases; but I will only relate one or two; not because they were the most successful, but because they occurred in relation to depositions, of which the removal, without operation, is either extremely rare or altogether unprecedented; and, also, as showing that I am tolerably exacting, in testing the correctness of *the principles* on which they were treated.

I would, however, guard the reader from one impression into which the foregoing observations might naturally mislead him; viz. that, if with great care and successful detection of the erring organ, &c. &c. cancer can be influenced in any way, —a fortiori, other depositions would be surely, and perhaps *readily*, brought under control. This is not exactly so. I believe many depositions, and, in fact, most, require quite as much pains as any malignant tumour; because in this the injurious influences and the erring functions are, so to speak, grossly palpable to a strict enquiry. Their correction is here the difficulty. In benign tumours, the correction, when discovered, is comparatively easy; but the smallness of the defect, sufficient to produce many ordinary and benign depositions, requires that the induction of facts should be just as strict as in any case of malignant tumours.

A gentleman, about fifty-four years of age, was brought to me, January 20, 1843, by an experienced practitioner (Mr. Debatt, of Islington), with a large tumour occupying the left side of the scrotum. He had been troubled with hydrocele, for which he had been repeatedly tapped, and about a pint of the usual straw-coloured fluid had been evacuated. He had, also, had the hydrocele injected some months previously; but, as would appear, unsuccessfully. The tumour he now showed me was evidently not hydrocele: it was exceedingly firm, very heavy, of rather large size (measuring twelve inches in circumference), and at first conveyed the sensation which would be imparted by a diseased testicle. Examining it, however, very carefully, and with attention to the points of diagnosis which I have recommended*, I gave the opinion that the testicle was probably sound; that the deceptive character of the tumour depended on an altered state of secretion into the sac of a much thickened tunica vaginalis; and that the only local procedure which I would recommend on the present occasion would be a puncture, both as testing the correctness of the opinion given, and as facilitating any further examination which might be necessary. The patient was fat and corpulent; he had a full, jerking pulse; furred tongue; irregular bowels; skin hot and dryish, and occasionally bloody evacuations from the bowels. He had an unhealthy countenance, a sort of pasty-white, shot, as it were, with yellow, occasional headache, and complained much of a disposition to sleep. In short, he appeared in such

* See Practical Directions for the Diagnosis of Inguinal Tumours, &c. &c. London, 1830.

indifferent health, that even the puncture appeared to me at the time of questionable propriety. However, as he wished for a more definite prognosis than I could otherwise give, I made a puncture and drew off about three quarters of a pint of very dark secretion, but so much like venous blood, that I thought it quite necessary to reserve it for examination. This secretion (for it was certainly not blood) was contained in a very greatly thickened tunica vaginalis; but, notwithstanding that the gland felt a little larger, yet I believed that, due allowance being made for the dense structure by which it was covered, it was really unaffected. I now told the patient that I thought he would do very well; to make himself comfortable (for he was exceedingly nervous), and that when his health was in an amended condition, I would institute some proceeding for the more effectual relief of his local complaint.

He was accordingly ordered small doses of blue pill and colocynth; and these were subsequently replaced by hydr. c. cretâ and antimony. He was desired to be moderate in his diet, and to take a little wine, but to discontinue beer. A catheter was passed a few times, which assisted in relieving some irritation of the urethra; but all these measures, to which were added evaporating lotions, effected no change in the tumour, which was as large as ever. His health had certainly improved; but his countenance was still bilious, and abdomen full. I now recommended that our endeavours should be directed chiefly to the liver, as I was convinced that that organ was principally in fault; but still only with the intention

of putting him in condition for the operation. I accordingly prescribed *occasional* single doses of calomel; but the means *ordinarily* employed for the purpose intended, were aperients combined with narcotics, almost any of which can be made very effectual excitants of the liver in certain proportions, which will vary in different cases. By these means we obtained large quantities of dark-coloured secretions from the bowels, considerable diminution of corpulency, and evident decrease in the size of the tumour. These measures were continued, and the dimensions of the tumour taken at each visit, until the whole was so nearly gone, that I discontinued my visits, begging Mr. Debatt to inform me of the result. At this time, very little fluid remained: the tunica vaginalis had nearly recovered its natural condition; and, so far as could be distinguished through the yet remaining fluid, the testicle appeared to justify the opinion first given. I have since heard from Mr. Debatt, who concludes his letter by saying—"March 30. Tumour entirely dispersed. There is a little thickening of the tunica vaginalis; but the testicle feels sound, and he experiences no inconvenience whatever, either in the testicle, spermatic cord, or scrotum. His tongue is clean, pulse regular, and bowels healthy. In fact, all his bad symptoms have left him."

A lady was brought to me, of about twenty-four years of age, with two tumours on the inside of the upper jaw: the one apparently bone, the other of softer material.

In the immediate vicinity of the tumours, there were the stumps of two decayed teeth; and this

naturally enough suggested that the irritation which they created might have been the cause of the tumours. On examining her mouth, I found that there was a small deposition on the opposite side, *where there were no stumps;* and I also observed that the tonsils had been the subject of chronic deposition. I therefore considered that, although the decayed teeth might have had something to do in determining the *site* of the more advanced depositions, yet that they were in no other sense the cause of them. I therefore determined, as she suffered no pain, to try what influence I could produce on the tumours without the extraction of the teeth.

I therefore took a careful account of her history; put her on the plan I thought best adapted to her condition; and told her to let me see her again in a week. When she came, I saw no amendment in any thing; but, on questioning her, I found that she had not been obedient, except as regarded her medicine. A relative, who was with her, expostulated with her very sensibly; and I told her that it was folly to come to me, unless she obeyed my directions. She now became an obedient patient; and the tumours in a few weeks began to diminish in size. At this time she left town, with my consent. During her absence, the tumours had still somewhat further diminished; and the diminution continued. Having thus, as I thought, satisfied the objects of enquiry into the nature of the case, I said I had no objection to the stumps being removed, since the absorption of the tumours might possibly progress more rapidly. They were

removed accordingly, by Mr. Cartwright, but without, so far as we could perceive, either hastening or retarding the progressive absorption.

I now said that she might discontinue her attendance, but persevere in her plan; and if the remaining portion of the tumours proved tediously slow in becoming absorbed, that I would remove them for her at any time. She accordingly left me. Shortly after this, a ridiculous circumstance occurred. She was staying with a friend, a short distance from town, when the medical attendant of the family being in the house, she showed him the tumours. He at once declared the one to be cartilaginous, the other osseous, and that they ought to be removed. As they had never heard these terms before, they became excessively alarmed; went off to the surgeon he recommended—an eminent man in London,—who at once removed them; with what ultimate result I have not been able to ascertain.

I had selected a few more cases; but I perceive that I have already exceeded the limits to which I proposed to restrict these observations; and I must, therefore, hasten to their conclusion.

Should I have succeeded in inducing people to think, and to apply the principles I have endeavoured to inculcate, it will encourage me to proceed to the discussion of particular varieties of tumour, when an opportunity will be offered for further illustration.

I will, therefore, only observe generally, that I know no form of deposition, to which the practice I would advocate has been fairly applied, which has

not been influenced thereby; and many of the cases have been very striking.

I have seen an enlarged thyroid gland reduced to its healthy condition, which had entirely resisted iodine and the (so called) specifics. I have, also, seen bronchocele yield, when combined with hypertrophy of the heart and an aneurismal tumour, close to the bronchocele; a case *suspected* at its commencement, and rendered clear by the diminution of the bronchocele.

I have seen depositions, of whose site there could, I think, be no doubt, as seated on the liver, influenced by means alone referable to the principles I advocate; and I see no end to the power we may gain over morbid depositions, if we will only take the trouble ordinarily bestowed on other scientific investigations.

To suppose that this is easy—that it can be learnt from books or lectures, which repeat, year after year, the practice of routine, and which, though well enough for certain examinations, are very different things from enquiries into nature,—would be, in my opinion, a mistake; yet there is no real difficulty, but industry, and that we ought not to grudge.

To expect that medical science should progress as fast as it otherwise would, by a system of study which, *usually*, demonstrably leaves out as many facts in the investigation as those of which it takes cognizance,—these last being often, too, the least important,—is as hopeless as in all other sciences it would be held to be ridiculous.

On the other hand, to allow the powers of medical science to be limited by any man's authority,

so long as such fulness of investigation only is denied to it, as is held indispensable in all other sciences, not only promotes all kinds of quackery, but is a slavery of mind, not merely unbecoming our exalted functions, but, in my humble opinion, a direct compromise of our duty to the public; whose ignorance or error, in regard to the physical laws of their being, it is our duty to enlighten; and whose sufferings is our province to prevent or alleviate, to the utmost extent of our power.

We must, however, first become acquainted with the laws ourselves, and the influences through which they operate. This is not to be done by means of anatomy *only*, or the mere results of disease *only* (morbid anatomy), but by an investigation of *all* the causes in operation; and until we discover better, I see not how we are justified in neglecting that mode which we *know* to have been successful in all other branches of knowledge: I mean, common sense, or Inductive reasoning. The most efficient means, however, of setting to work in this way, in a manner co-extensive with its importance, are to be found, so far as London is concerned, in institutions too much under the influence of monopolists, who seem to think a very different kind of Induction much more attractive. Perhaps in this, after all, they are not singular; for, if we look around us, we shall find that this "metallic traction" is not confined to surgical or any other science, but that we might almost say, with Horace,

> "*Omnis* enim res.
> Virtus, fama, decus, divina, humanaque pulchris,
> Divitiis parent."

FINIS.

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